A FEW CHAPTERS IN ASTRONOMY.

CLAUDIUS KENNEDY.

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BY

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LONDON:
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
1894.

ALERE FLAMMAM.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

PREFACE.

THE mathematical discussions in this little book are quite elementary, and geometrical in character, except that, in three instances, two differentiations and an integration of the most rudimentary kind have been used. In a very few cases, the results of analysis have been simply accepted; and even of these, few as they are, some are given only to verify conclusions already arrived at independently.

November 15, 1894.

CONTENTS.

CHAPTER V.

ON THE POSITION OF THE DYNAMICAL HIGH TIDE RELATIVELY TO THE CELESTIAL TIDE-PRODUCING BODY.

CHAPTER VI.

THE "HORIZONTAL" PENDULUM.

A FEW CHAPTERS

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ASTRONOMY.

ERRATA.

Page 25, line 6 from bottom, for FDH read FHD.
" 26, line 2, for HF read DF.
" 30, line 3, for *K* read *C*, twice.
" 32, line 7 from bottom, for r^2 read r.
" 56, line 19, for (Note) B read C.
" 130, Note G.—The relative revolution of the sun round the earth would prevent the ellipticity from increasing beyond a certain limit.

whose centre is at our point of view.

But ordinary observers, and even astronomers themselves, are not in the habit of referring terrestrial objects around them to the sphere of vision. Such objects are referred to what writers on perspective call the *plane* of vision at right angles to the line of sight, which the eye, as it were, always carries about with it. There are different reasons for this. The idea of the plane of vision is, in some respects, simpler than that of the sphere of vision, and presents itself more immediately to the observer; and this the more readily as it is always of limited angular extent. Besides this, all ordinary drawings and pictures are made on plane surfaces, for different obvious reasons.

A FEW CHAPTERS

IN

ASTRONOMY.

CHAPTER I.

ON A VISUAL ILLUSION AFFECTING CERTAIN ASTRONOMICAL PHENOMENA.

In considering the visual positional relations of the heavenly bodies to each other and to various given lines and planes, it is of course necessary to regard them as projected on an imaginary spherical surface whose centre is at our point of view.

But ordinary observers, and even astronomers themselves, are not in the habit of referring terrestrial objects around them to the sphere of vision. Such objects are referred to what writers on perspective call the *plane* of vision at right angles to the line of sight, which the eye, as it were, always carries about with it. There are different reasons for this. The idea of the plane of vision is, in some respects, simpler than that of the sphere of vision, and presents itself more immediately to the observer; and this the more readily as it is always of limited angular extent. Besides this, all ordinary drawings and pictures are made on plane surfaces, for different obvious reasons.

When the eye is stationary the angular extent of distinct vision is quite small. Even if the eye be allowed to move, while the head still remains stationary, the angular range of vision, or the extent of the field of view which can be attained without too much disturbing effort, though much greater than before, is still small; and therefore in such cases the difference between the plane of vision and the sphere of vision may be practically of very little importance.

But it would be otherwise if the plane of distinct vision could be made larger, for then its own perspective would sensibly affect the question. We need not, however, go into this; for if we would compare two objects whose horizontal angular distance is too great for them to appear in the same limited field of view, we turn the head, or perhaps the whole body, round a vertical axis from one to the other; and we see each by turns in its own separate limited plane of vision, and usually with a very indistinct idea of the geometrical relations between those different planes. This is the main cause of the illusion now in question.

We may just mention here the most striking instance of such illusion, returning to it further on, for explanation. It has the advantage of being easily observed every month. If the crescent moon be not less than two or three days old, the sun being near setting, the middle of her illuminated limb, which, of course, is turned directly towards the sun, will seem to point decidedly above the sun. As the angular distance of the moon from the sun increases, the apparent discrepancy becomes more marked, and, as the present writer happens to know, competent astronomers and mathematicians, who of course are perfectly aware of the real conditions of the case, will acknowledge that they cannot divest themselves of the feeling that they actually *see* the middle of the moon's illuminated limb to be pointing several degrees above the sun.

The unequal raising of the sun and the moon by refraction has no share in producing the illusion; for its effect is to raise

the sun, which is at the horizon, more than it does the moon, which is higher in the sky, and so to diminish the illusion to a slight extent.

Every great circle of the sphere of vision is, to the eye viewing it simply and unconnectedly, a right line, because the eye is in the plane of it. The horizon is such a great circle, and it is to the eye a right line; a straight edge held in the hand can be applied to it and will fit at any place. Not only this, but the horizon presents itself to the *mind* as everywhere a right line; for a reason which we shall mention presently.

But if we could draw any other great circle on the sphere of vision, or on the vault of heaven, though it, also, would be to the eye viewing it unconnectedly perfectly straight in every part, it would intersect the horizon in two opposite points, while having some elevation above the horizon at its middle part; therefore to the *mind* of the beholder, who is so habituated to dealing with lines &c. as drawn on plane surfaces, and knows that two right lines, as existing on such surfaces, cannot meet in two points, that line would present itself as an arch. Its inclination to the horizon would be continually varying, as it was followed by the eye from end to end. A person standing opposite the middle of a very long, straight, horizontal, architectural feature, or other such line, of sufficient height, can only with difficulty divest himself of the idea that as he turns his head from side to side, he *sees* that line as a curve with its concavity downwards; since near him it presents itself as horizontal; but on each side as sloping down to the horizon.

If we suppose a straight line to be traced on the sky at a considerable altitude, we should not refer its direction, at any point, immediately to the horizon, perhaps quite out of sight. But we should do what would be equivalent: that is, refer its direction, at any point, to the vertical at that point; and we always have a pretty accurate consciousness of the vertical from the sense that we have of the direction of gravitation. As we

consider, in succession, at not too great an altitude, the imaginary vertical great circles which we trace for ourselves in succession while turning the body round, we take them as parallel to each other, because they have been similarly related to the successive outlooks. But they are actually converging; the consequence is that a straight line or great circle traced across them would cut them at very perceptibly different angles; even within a comparatively short distance. It cuts at different angles lines which in one sense have the same direction, or are parallel after their own fashion, being all at right angles to the horizon, and therefore it seems to be curved.

But why does the horizontal great circle look straight, whilst another inclined to it looks curved; both being straight? The reason is that, in looking round, the observer turns his head, or his whole body, on a vertical axis, so that every point of the horizon has the same directional relation to his outlook, as he faces it standing erect. The horizon, therefore, naturally becomes the most general line, or plane, to which the position of an object is referred. But this is not the case with the other great circle. As the beholder turns round on his *vertical* axis, which is inclined to the plane of that great circle, every point of that circle has a different relation of direction to his outlook, as he stands erect; consequently when the eye is made to run round it in the ordinary way it seems curved.

But if, while holding himself up straight, he were bound to a post which then was inclined until it was at right angles to the plane of the great circle now in question, and if the post were then rotated on its axis, the familiar horizon being hid from view, that great circle, as he was turned round, would appear to him to be straight, for the same reason that the horizon ordinarily does so; and if he fixed his attention very strongly on that line, then the horizon, if uncovered, would seem to him to be curved as he was being turned round. That this would be so can be proved by an easy experiment. Stand near the corner of a long enough room, or lobby, or passage, and view the opposite cornice,

or the juncture of wall and ceiling, running along the length of the room &c. The cornice will be everywhere straight to the eye; yet its visual inclination to the horizon increases continually as the eye follows it from the nearer to the further end. Hold a straight rod vertically, with both hands, so as to visually cross the cornice; now while the arms remain rigid and unmoved, in order that the rod shall be always vertical, turn the whole body from side to side briskly enough, keeping the eye and attention strongly fixed on the visual intersection of rod and cornice; and the cornice will appear to be curved with the concavity downwards, on account of the continual change of its visual angle of intersection with the vertical rod. A few trials may be necessary in order to catch the effect, as it is considerably diminished by the knowledge that the cornice is straight, and by the observer's involuntarily comparing consecutive portions of it with each other; which latter cannot be done in the case of an imaginary straight line on the sky.

To return to the case of the crescent moon referred to already. Suppose that our latitude is nearly that of London, say $51\frac{1}{2}°$, and that the young moon is 45° from the sun, which is setting; and, to obtain a mean condition, let the time be an equinox and the moon on the celestial equator; the altitude of the moon will be 26°. If the straight line along which the middle of her illuminated limb points towards the sun could be traced on the sky, its inclination to the horizon would be, of course, at the sun $38\frac{1}{2}°$ (the complement of the latitude); but at the moon it would be about $29\frac{1}{2}°$. That straight line therefore is steeper at the sun than at the moon by 9°. If persons who had not considered the matter, and even some others, when off their guard, tried to trace that line by the eye, they would start from the moon at a a downward slope of $29\frac{1}{2}°$, and preserve that slope as well as they could, until reaching the horizon; just as they would do if dealing with a straight line on a plane surface directly facing them. This of course will carry them many degrees above the

sun. But if the observer were in some unaccustomed attitude, say half reclining and looking obliquely over his shoulder, so as to obscure his sense of the vertical or horizontal direction, and if all known horizontal and vertical lines were properly concealed from view, and if he had a good eye for straightness and symmetry, he would doubtless be able, having started in the proper direction from the moon, to continue his trackless course until hitting off the sun.

Perhaps the simplest, and for some persons the most striking, exhibition of this deception would be when the moon is in the first quarter, or "half moon," and the sun is setting. Suppose the altitude of the moon to be, at the time, m degrees. The terminator, or boundary of light and shade on the moon, is straight and vertical, and the middle of the illuminated limb is pointing horizontally, and yet directly at the sun which is setting m degrees lower down. If we try to follow by the eye the direction in which it points, we shall be tempted to trace for ourselves an imaginary line on the sky everywhere horizontal and having always the same distance from the horizon, as we should do in a diagram on a plane surface; and the result will be that our production of a line, which really points directly at the sun, will pass m degrees above the sun. (Such a line, if traced on the sky, would be a small circle of the celestial sphere, and, paradoxical as it sounds, everywhere convex towards the straight horizon.)

In this case the illusion is obvious, and felt at once to be something that requires explanation; besides which it is not calculated to lead to any ulterior mistake.

But there is another exhibition of this illusion which is not of so innocent a character; it does not manifestly betray itself as an illusion, and it has given rise to misconception. It is a seeming phenomenon which by ordinary persons is not considered to require explanation, because it appears at first sight to depend so evidently on another principle. Even those who

must be aware of the actual circumstances in this case, have not, so far as we know, given any warning on the subject, at least in print.

Every one must have noticed what seems like the well-marked curvature of the path of an ordinary meteor or shooting-star, whether a sporadic one, such as may be seen on every clear night, or one belonging to a shower, provided its apparent path be not too near the vertical. This apparent phenomenon was, as the present writer can testify, very strikingly displayed (if this be not a bull) by the shower of Andromedids *, or Bielids, on November 27, 1872. Certain others also remarked the same, as anyone must have done. Any *pictures* (not diagrams on a star map) that we see of meteoric showers invariably give a decided curvature, concave downwards, to the luminous tracks. To most persons there is no difficulty about this; but quite the contrary. One of the observers just referred to, speaking of that shower of Andromedids a couple of days after its occurrence, remarked how interesting it was to see the curvature of the trajectories of the celestial projectiles due to gravitation.

But a moment's consideration will show that this is quite a mistake. The nearest point of any of these visible tracks was probably not less than forty-five miles distant, the track itself being many miles in length. Now the very longest period of visibility that we can allow to any of those meteors is two seconds, in which time one of those bodies would fall, considering the resistance of the air, less than 64 feet. But a linear deflection of 64 feet would be quite insensible to the eye in such luminous tracks as we have mentioned; even supposing it to be at right angles to the apparent track, which will but seldom happen. The case, of course, is quite different of a large meteorite which is seen by an observer to fall to the ground, not far off, after having been visible for a longer time. The illusion now in question is clearly due to the constant change of the

* These are sometimes called "Andromedes," as though the name of the constellation were Andromè.

inclination to the horizon of the sensibly straight luminous tracks of the meteors.

It is true that, unless the direction of the motion of a meteor is parallel to that of the earth, when the meteor enters the earth's atmosphere the resistance of the air will not only produce a violent retardation of its velocity, but will cause a deflection and curvature in the path of the meteor relatively to fixed space. But this curvature *will not be visible to the observer*. This is easily seen thus:—Suppose the meteor to be visible, even before entering the atmosphere, the observer would *see* only its motion relative to the earth, the air, and himself, all regarded as stationary; when the meteor, with this apparent motion, enters the apparently stationary atmosphere there is nothing to cause any perceptible change in the direction of its motion; no curvature visible to the beholder will be produced by the resistance of the air in the path of the meteor. (Nor will there be any change in the position of the apparent radiant produced by said resistance. We mention this because the contrary has been directly contended for.)

The reason why the seeming curvature in a meteor's track is not greater than it is seems to be this, that the eye is not only comparing the track with the vertical, or the horizontal, at every point, but it is also to some extent comparing contiguous lengths of the track with each other; and this tends to correct and diminish the illusion.

For this reason the more rapid the flight of the meteor, the less will be the appearance of curvature in its path, for in such cases the visible path approaches more nearly to the condition of a luminous line seen at once from end to end, the parts of which can be more readily compared with each other. This was well illustrated by many of the quick-moving Perseids of August 10, 1883 *.

* There is a detail of this illusion which is worth mentioning. It appeared to be very noticeable with a large proportion of the meteors of Nov. 27, 1872. Near the end of visibility, the apparent downward curva-

The illusion of which we now speak may easily lead some persons into error when endeavouring to fix upon the radiant point of a very sparse meteoric system.

If the insufficiently-experienced observer has not been fortunate enough to catch with his eye any of the few meteors pretty near to the radiant point, he will, in producing the visible parts of the meteor-tracks backwards, almost certainly pass above the radiant, and so fix its position higher than it ought to be. Or, if on the look-out for meteors belonging to a certain known radiant, he might easily refer thereto some sporadic meteors really coming from a different origin at a lower altitude, when perhaps it might be important to know that, in fact, none belonging to the radiant were to be seen on that night.

Conversely, when endeavouring to fix the position of a *visible* but very faint object, say a new comet, by using alignments with known stars at considerable angular distances from the object, he may easily do the opposite; that is, assign to it a position lower than the true one. From his alignments the very faint object might be found again on the following night by *himself*, though perhaps not by another, whose skill in allowing for the illusion now in question might be either greater or less than his.

This illusion might, with some persons, slightly affect the apparent curvature of a comet's tail, if very long. Some years

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ture of the meteor's path seems to increase somewhat rapidly, as in the ballistic trajectory of a projectile, caused by the resistance of the air. This also is represented in some *pictures* of meteoric showers. But though gravitation, with the resistance of the air, would really produce such an effect, it is utterly impossible, for the reason given above, that it could have been perceptible to the eye. The deception may be due, in some way, to the fact that the eye is following the apparently curved path of a luminous point whose velocity is being, for two distinct reasons, ever more and more rapidly retarded. This seeming phenomenon gives rise to another misapprehension. It makes the meteors look, at the end of their luminous tracks, as though they were no farther off than the falling stars of a rocket.

ago there was a difference of opinion between two correspondents in a popular scientific periodical respecting the curvature of the (long) tail of the great comet of 1882. This was, in all probability, produced by the cause above mentioned. This might be not unimportant, in view of the conclusions as to the composition of comets' tails drawn by Bredichin and others from their curvature in connection with the known motions of the comets. But a comet's tail, being a visible and permanent object during the observation, so that different parts of it can be compared directly with each other, is much less liable to be affected by the illusion now in question.

CHAPTER II.

THE EFFECT OF THE EARTH'S ROTATION ON CERTAIN MOVING BODIES.

It was believed by Aristotle and by Ptolemy that the earth's rotation, if it existed, should affect the motion of certain freely moving bodies. Galileo also perceived that this must be so, while rejecting the particular effects contemplated by them, at least by the latter. (See Chapt. III., Note A.) Newton was the first to point out that freely falling bodies must deviate to the east of the vertical, on account of the rotation of the earth; and he suggested that experiments should be made with these in order to obtain direct proof of that rotation. Such experiments were tried by Hooke, in 1680, but with an insufficient height of fall. In 1836 Edward Sang, C.E., of Edinburgh, showed that the earth's rotation could be demonstrated by means of what is now called the gyrostat; but he did not carry out any experiments therewith. In 1837 the subject was discussed, in connection with the flight of projectiles, by Poisson. It came much more prominently before the general public when Foucault exhibited his famous Pendulum to the French Academy in Feb. 1851. Shortly afterwards he devised, for himself, and actually performed, the experiment with the gyrostat which had been proposed fifteen years before by Sang.

A common and popular explanation of the deflection of projectiles, currents of air, &c., from the rotation of the earth, is that if, in our N. latitudes, a body be moving southwards it is all the while passing over ground which has a greater velocity eastwards, from the rotation of the earth, than the

ground which it has started from, or has lately crossed, and that therefore it is left behind a little towards the west, or the right hand, by the surface of the ground beneath it; and that, for corresponding contrary reasons, when moving northwards it will gain on that surface towards the east, or still to the right hand. This is, of course, perfectly true; but the particularization of the meridional direction is often intended to imply, what is sometimes directly declared, viz., that the above statements are not applicable to bodies moving in the east and west direction. It is strangely forgotten that if a point on the solid ground south or north of an observer is moving towards his left, when he faces it, relatively to him as centre, with a certain angular velocity, a point on the ground east or west of him must be doing the very same; and that therefore a sufficiently free *horizontally*-moving body must be left behind, to the right in N., and to the left in S., latitudes, in whatever azimuth direction it may be going; and that, other things being equal, its apparent deflection must be the same for all azimuths of motion.

The period of the earth's rotation is, of course, a sidereal (not a solar) day; this contains 86,164 seconds of mean solar time. The angle described in one second of solar time is, then, 360°/86,164, or 15·04 seconds of arc, which in circular measure is $2\pi/86{,}164$, or $1/13{,}713$ *; this then represents the earth's angular velocity of rotation, which we shall denote by ω.

The resolution and composition of rotations is among the first elements of rigid dynamics. The two components of the earth's rotation with which we are now concerned are V, or that about the vertical line at the locality in question as axis, whose angular rate is $\omega \sin \lambda$, λ being the latitude of the place, and M, or that about the horizontal meridional line at the locality as axis, whose angular rate is $\omega \cos \lambda$. (See NOTE A.)

* It is interesting to note that 13,713 is, itself, the mantissa of its own logarithm to five decimal places. But we need not attach any mystical significance to this coincidence.

We may give here a practical illustration of the existence of these two components of the earth's rotation. If in N. latitudes a star close to the horizon be observed with a telescope whose eye-piece is furnished with a micrometer scale, the star will be found to have a motion in the horizontal direction towards the right (whatever vertical motion it may have compounded therewith); and this horizontal motion will be found to be the same for all stars close to the horizon in whatever azimuth direction they may be: and the angular rate of the horizontal motion will prove to be that of the earth's rotation multiplied by the sine of the latitude; this being due to the earth's component rotation V. Similarly, if one observes any stars close to the prime vertical, or the great circle passing through the zenith and the E. and W. points on the horizon, he will find that they all have the same rate of motion along that great circle (whatever other motion they may have compounded therewith); and this angular rate of motion along the prime vertical will prove to be that of the earth's rotation multiplied by the cosine of the latitude; this being due to the earth's component rotation M.

Of sufficiently free bodies, those which are moving horizontally are affected by the component V, by which the surface of the ground at the place of observation rotates in its own (instantaneous) plane. Those which are moving vertically, whether upwards or downwards, are affected by the component rotation M, by which the surface of the ground is always being tilted over eastwards.

We shall first consider those which are influenced by the component V. It may be best to begin with an imaginary case, for illustration. Suppose a body started to slide on a perfectly frictionless, even, horizontal surface, or floor, in a vacuum. If the floor were stationary the body would, of course, describe, from inertia, a right line thereon with a uniform velocity v. But as the floor is always rotating in its own (instantaneous) plane

with the angular velocity $\omega \sin \lambda$, and as there is no connection between the floor and the body which would make the latter partake of the rotation, it will not do so; but it and its radius-vector will be left behind, and that line, if visible, would appear to rotate about the point of starting, watch-wise in N. latitudes, and counter-watch-wise in S. latitudes, with the uniform angular velocity $\omega \sin \lambda$, while being itself described by the body with the uniform linear velocity v. Consequently the body would describe about the point of starting, as pole, a spiral of Archimedes, whose equation would be $r = \frac{v}{\omega \sin \lambda} \theta$. If the body were started from the middle of the floor, with so small a velocity that it would not reach the edge of the floor for a few days, it would present the curious phenomenon of revolving (with an ever widening orbit) round the point of starting for no apparent reason. We must, however, content ourselves with the consideration of masses moving horizontally under more ordinary conditions.

The winds afford a familiar instance. The explanation of the direction of the trade-winds and cyclones is now pretty generally known, and needs only to be mentioned. The greater heating of the air by the sun in the neighbourhood of the thermal equator causes that air to ascend, which occasions an indraught of the lower air both from the N. and from the S. For a non-rotating earth, the general direction of these would be meridional; but the rotation of the earth causes an apparent turning to the right on the north side, and to the left on the south side, of the equator; thus producing the N.E. trades on the north side, and the S.E. trades on the south side, of that line.

A sufficient local extra heating of the air in N. latitudes causes, in a similar way, an indraught of the lower air from all sides; the component rotation V causes the converging masses of air to pass in N. latitudes to the right of the centre of the superheated area, which produces a vortex turning in the opposite direction to that of the hands of a watch lying face upwards on the table; the corresponding result in south latitudes being a vortex

turning the other way, or with the hands of the watch. Such vortices being called cyclones.

Ocean currents must be very considerably affected by component rotation V; but these are subject to a variety of other important influences of which we shall mention only prevailing winds, land barriers, and mutual interference. It would be generally impossible to distinguish the effect now in question from others, and useless to speculate thereupon; except perhaps in one apparently simple instance, with which, as it happens, we are practically concerned. It can hardly be doubted that it is largely in consequence of component rotation V that the warm Gulf Stream bears so strongly on the coast of north-western Europe. It may be worth while to advert to the following:— There are five great ocean vortices. The two in N. latitudes, viz., that in the N. Pacific and that in the N. Atlantic, both turn watch-wise. The three in S. latitudes, viz., that in the S.E. Pacific, that in the S. Atlantic, and that between S. Africa and Australia, all turn counter-watch-wise. It seems highly probable that all this is, at least, promoted by the earth's rotation. If a great ocean vortex were due to an extensive current movement of the water, produced somehow under the condition of the earth's rotation, the direction of its turning would be such as we have just mentioned, and opposite to that of a cyclone in the same latitudes produced as above described.

The course of the flight of migrating birds is probably sometimes affected by component rotation V. But as the consideration of their case would involve some speculation, let us propose to ourselves another one which might actually occur.—The keeper of a light-house several miles out from the coast has some homing pigeons, bred by himself, which are well acquainted with the district. One is let go from a point on the coast; it starts at once to return directly to the light-house; the bird is guided solely by his sight of the light-house, and the water being perfectly smooth, he is without means of knowing that he is always edging sideways to the right of the instantaneous straight line

from himself to the light-house; he will keep his head always pointed directly towards the light-house, and to do this he must be continually turning very slowly towards the left, doubtless without perceiving that he is doing so. The forward velocity of his flight is uniform, and his involuntary sideward motion to the right will go on increasing, until the resistance of the perfectly still air to that motion becomes great enough to prevent any further increase therein; the sideward velocity then has reached its final magnitude, and becomes constant, like the forward velocity. The bird's visible course, or that relative to the surface of the earth, will then become, *quam prox.*, a logarithmic spiral described backwards towards its pole, which is at the light-house. (See Note B.)

Let A be the point at which this has taken place. If now the latitude be 51° 30′, that of London, and the distance from A to the light-house be 10 miles, and the bird's velocity of flight be at the mean for such cases, say 800 yards per minute, or 40 feet per second, and his weight 14 oz., and the coefficient of sideward shifting 9·8, which we have good reason to believe is pretty nearly correct; then his greatest departure to the right of his intended straight line of flight from A to his home will be just about 70 yards, at the distance of 3·68 miles from the light-house. If this departure seems somewhat small, let us remember that it has taken place in spite of the bird's constant (unconscious) endeavour to avoid it, and in spite of the lateral resistance of the air.

Probably there is always a sensible deviation of this kind when a bird is travelling to a sufficiently distant intended goal. His flight, however, being generally over the land, the sight of the more prominent objects in view would make him more or less aware of his sideward shifting, and thus suggest to him to make some allowance for it by directing his head to the proper side of the goal, or the left in N. latitudes; but the amount of angular allowance necessary would depend on the velocity of flight, and on the latitude, and also on the bird's own weight and his coefficient of sideward shifting; and it seems very unlikely that

instinct, much as it can do, would enable him to make due allowance on account of these; though it would doubtless enable him to provide against a cross wind.

We see, then, that the familiar expression, "As straight as the crow flies," should not be lightly used, without distinctly postulating the condition that the bird is making due correction for the rotation of the earth.

In the case of a railway engine and train running along a perfectly straight reach of the line, the rails being perfectly level with each other, the sideward shift is prevented by the resistance of the right-hand rail in N. latitudes, and of the left-hand rail in S. latitudes; and it is said that the right-hand rail and the flanges of the right-hand wheels get more wear in N. latitudes, on this account, than the others.

This is undoubtedly so. We know already (see NOTE B) that the expression for the pressure P against the right-hand rail is

$$P = \frac{2v\omega \sin \lambda}{g} W; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which v is the velocity in feet per second and W the weight of the moving body (see NOTE B). For an engine going at 30 miles an hour, or 44 feet per second, in the latitude of London 51° 30′, this would be 1/6410th part of its weight, and if this weight were 30 tons the whole pressure would be 10·48 lb.; and this would have to be distributed among all the right-hand wheels. The effect, then, is so small that it must be undistinguishable; as it would be altogether overborne and masked by a gentle cross wind, or by a difference of level between the rails, say 4·71 feet apart, of only 1/100th inch; or by a gentle curve in the line of 60 miles radius; not to mention other causes of unequal wear on the two rails.

It is said also that rivers in N. latitudes must, for the same reason, wear away their right-hand banks slightly more than their left ones. This is undoubtedly true; but the effect is utterly imperceptible; not only because the greatest velocity of a river flow is relatively so small, but also on account of the

incalculably greater effect of various other causes of inequality in the erosion of the banks.

Among moving bodies influenced by V must be mentioned the famous Foucault's Pendulum; but this is deserving of a chapter to itself, which we shall give it.

We now come to moving bodies which are affected only by the earth's other component rotation M, that is to say those moving vertically, whether upwards or downwards. Just as in N. latitudes, a sufficiently free body, projected or moving away horizontally from before a spectator standing vertically, will deviate towards his right, so if gravitation could be neglected, to a spectator lying horizontally in the meridian in N. latitudes, with his feet to the south, a body projected away from before him in the plane of the prime vertical will deviate to his right, owing to the rotation of that plane in itself with the angular velocity $\omega \cos \lambda$. Gravitation alters the case, except for a vertical discharge or movement. If the observer lie on his back and discharge the projectile vertically upwards its deflection towards his right is one to the west. If he lie face downwards, say at the edge of a mural cliff, and discharge, or simply drop, the body downwards, its deflection towards his right is one to the east.

We shall here discuss the latter, viz., a body dropped from a height. That such must deviate to the east of the plumb-line is easily seen otherwise thus. A point on the surface of the ground is moving eastwards, from the rotation of the earth, with the linear velocity $R\omega \cos \lambda$, R being the earth's radius; but a point directly over it, at the height h, is moving eastwards with the velocity $(R+h)\omega \cos \lambda$. The latter is therefore moving eastwards faster than the former with the additional velocity $h\omega \cos \lambda$; consequently a body simply dropped from the upper point will, at its fall, have left the lower point behind it towards the west. This deviation, δ, of the body towards the east, if the resistance

of the air to the falling body be neglected, is given by the equation

$$\delta = \tfrac{2}{3} ht\omega \cos \lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is, for same h, one fourth of the *Westward Shift* in the Chapter on the Deviation of Projectiles (since, for same h, this t is half the other). (For proof see NOTE C.) It being in a vacuum $h = \tfrac{1}{2}gt^2$; and the above expression for this deviation can be written $\tfrac{2}{3}\sqrt{\dfrac{2h^3}{g}}\,\omega \cos \lambda$, as it usually is.

A body dropped from a height must have also, as is evident, and as was pointed out by Hooke, a very small deviation towards the south; *this* is not produced by M, but by the horizontal (southward) component of the centrifugal force of the earth's rotation being greater at the top of the height of fall than at the ground (except at the equator, where both are zero). Its magnitude, which is easily obtained geometrically, is only $\tfrac{1}{8}ht^2\omega^2 \sin 2\lambda$, or $\tfrac{1}{4}\dfrac{h^2}{g}\omega^2 \sin 2\lambda$; neglecting the resistance of the air. The presence of ω^2 in it shows, at a glance, that it must be excessively small for all practicable experiments. In that of Guglielmini, mentioned below, it would be less than 1/30,000th of an inch. (The formulæ now given agree with that of Prof. Bartholomew Price obtained analytically.) If, in the analytical discussion of the deviation of a falling body from the vertical, quantities of higher (*i. e.* smaller) orders of magnitude than the first are neglected, this component of it does not emerge into view. A body *projected* vertically upwards is not affected in *this* manner, either in its ascent or descent.

Experiments have been carried out by various persons to detect the deviation of falling bodies from the vertical owing to the rotation of the earth. For instance by Guglielmini, in 1792, in a tower at Bologna (lat. 44° 30′), with a height of fall of 241 feet; by Benzenberg, in 1803, in a tower at Hamburg (lat. 53° 33′), with a fall of 234 feet, and, in 1804, in a coal-mine at Schlebusch, Westphalia (lat. 51° 25′), with a fall of

262 feet; and by Reich, in 1832, in a mine near Freiberg, Saxony (lat. 50° 53′), with a fall of 488 feet. These experiments, especially those of Reich, were, as far as regards the eastward deviation, satisfactory, considering the delicacy of their nature and the great difficulty of avoiding various causes of inaccuracy, some of which could produce disturbances often very much greater than the deviation to be determined. (See NOTE D.)

It is self-evident that, the height of fall being given, the astward deviation will be greater if the time of falling can be made so in a proper manner. Therefore, for given h, the eastward deviation is greater in resisting air than in a vacuum. (But we shall find in Chapt. III. that this last is not the case with the westward deviation of the point of fall of a body discharged vertically.)

This suggests a more convenient method of carrying out such experiments as the above. By making the falling body descend slowly enough we can obtain an eastward deviation, δ, large enough to be satisfactorily determined, with very much smaller heights of fall than those mentioned above. The falling body might be a sort of parachute, very easily designed and constructed, which, like a shuttle-cock, would be kept rotating about its vertical axis by the resistance of the air. If this were such that it would descend with a uniform velocity, v, of three feet per second, it would have, with a fall of only 80 feet in the latitude of London, a deviation, δ, to the east of just over one inch, allowing a little for the lateral resistance of the air. This deviation is 17 times as much as if the fall had been in a vacuum, and probably 14 times as much as that of a bullet let fall in air. In this case the equation is

$$\delta = ht\omega \cos \lambda \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

the 2/3 of equation (2) being now unity (see NOTE E). Since t is here h/v, this value of δ is $\frac{h^2}{v}\omega \cos \lambda$. Therefore, for the same parachute, the deviation varies as h^2. Observe the last paragraph of NOTE E.

The parachute might be an inverted cone, about three inches in diameter, composed, say, of tracing-paper, and furnished with two very small wings opposite each other and set obliquely so as to cause rotation. If the vertical angle of such cone be 90°, or a little less, it will descend steadily with the velocity mentioned. It should, of course, be made to descend within a chimney-like box, to protect it from movements of the air; and this should be in a suitable place inside a building; so that there might be no convection currents within the box, caused by inequalities of temperatnre on different sides of it. It would probably be impossible, except by accident, to make the parachute so symmetrical about its axis that it would not be slightly deflected from its proper line of fall by the resistance of the air. But because of its rotation it would descend in a cylindrical helix of very small diameter, the axis of which would be the mean line of descent and the actual line in a vacuum. If a large enough number of experiments were instituted, in which the parachute was made to start with the same side in different azimuths, the small errors arising from the semidiameter of the helix would be self-compensating. The very small lateral resistance of the air would, of course, slightly diminish the lateral deviation from the rotation of the earth.

A free pendulum (that is one free to swing in any direction, like Foucault's Pendulum, and unlike a knife-edge pendulum, or that of a clock) is affected, as to its rate of oscillation, by its sharing in component rotation M. It is, whether it be hanging at rest, or oscillating, rotating about the meridional horizontal line through its point of support, with the angular velocity $\omega \cos \lambda$. There is, therefore, a downward centrifugal force, as we shall express it, acting on the pendulum at its centre of mass, which, taking the mass as unity, is $r\omega^2 \cos^2\lambda$; r being its mass-radius, or distance of the centre of mass from the axis of rotation. It is evident that if the plane of vibration be E. and W., this centrifugal force, though apparently conspiring with g, will not increase the rate of vibration, because it is always directed

along the pendulum rod; it is not parallel with the direction of g, except incidentally, at the instant when the pendulum is at the lowest point. Consequently the period of a free pendulum swinging E. and W. is not affected by its rotation with M But if the plane of swing be in the meridian, the centrifugal force due to the rotation of that plane about the horizontal N. and S. line through the point of suspension *is* always parallel to the direction of g, and not in the line of the pendulum rod; except at the instant when the pendulum is at the lowest point. It is always proportional to the distance of the centre of mass from the said axis of rotation; but if the amplitude of swing of the pendulum be very small, as it ought always to be in the scientific use of the pendulum, this never differs sensibly from r. The pendulum, therefore, is oscillating, not simply under g acting at the centre of mass, but also under the parallel, conspiring, and sensibly constant centrifugal force $r\omega^2 \cos^2\lambda$, acting at the same centre (the mass is still taken as unity). It is very easy to see that if the plane of vibration be not in the meridian, but inclined thereto at the azimuth angle z, we shall have for the time t of the vibration of the free pendulum, not

$t = \pi\sqrt{\frac{l}{g}}$, but (see NOTE F)—

$$t = \pi\sqrt{\frac{l}{g}}\left\{1 - \frac{r\omega^2}{2g}\cos^2\lambda\cos z\right\}, \textit{ quam prox.} \quad . \quad (4)$$

If then the free pendulum's radius of oscillation l be that of a seconds pendulum, it will gain, in consequence of its own rotation with M, $\frac{r\omega^2}{2g}\cos^2\lambda\cos z$ sec. in every swing. It being a Foucault's Pendulum, its plane of vibration will rotate relatively to the material surface of the ground once in 24 sidereal hours $/\sin\lambda$. Therefore its rate of gaining is constantly varying from zero to its maximum, and back again, with a period of 12 sidereal hours $/\sin\lambda$.

If the pendulum is oscillating meridionally at the equator

(where it will retain its azimuth of oscillation), so that the gain shall be greatest, and if r be 37 inches, which is perhaps a fair mean value of it, the gain will be at the rate of one second in 125 years. Of course the practical unimportance of this does not detract from its dynamical interest. At the poles of the earth, where $\cos^2 \lambda$ vanishes, the vibration period of the free pendulum is unaffected by the rotation of the earth.

We now come to moving bodies which are affected by both components, V and M, of the earth's rotation.

Some of the movements of the atmosphere and of the ocean must be modified by V and by M at once; each making its own special contribution to the whole effect.

There is a phenomenon which must be largely due to both components of the earth's rotation acting together as auxiliaries. There would appear to be, in equatorial regions, a continuous current from E. to W. in the upper parts of the atmosphere, at the height of 20 miles or so. The peculiar sunsets which began with the great eruption of Krakatoa, in 1883, passed thence successively westward round the equator. It was evident that their cause was, at first, of limited extent, and that it was travelling in the direction mentioned. Before it became too diffused and widely spread, several passages of it round the equator could be distinguished, showing that it completed the circuit of the equator in about 13 days. It seems impossible to account for this but by the great cloud of fine dust from that unusually violent eruption; such dust being known to be capable of producing such effects. That dust must have been carried by a continuous current in the upper air over the equator from E. to W., at the rate of 76 miles per hour. It is obvious, from what we have seen respecting the trade-winds, that V and M would both conspire to produce this current, helped, no doubt, by the daily revolution round the earth of the sun's heating effect on the atmosphere.

We now turn to the pendulum swinging on knife-edges. This

is affected by M, as to its rate of oscillation, precisely in the same manner as the free pendulum, considered above, which has for the instant the same azimuth of oscillation; but, unlike the latter, its rate is affected by V also. The plane of its oscillation rotates about the vertical line through its position of rest with the angular velocity $\omega \sin \lambda$. This produces, in this pendulum, a centrifugal force directed away from the pendulum's position of rest, and opposing gravity. Then for very small amplitudes of oscillation, we have for the time t of the knife-edge pendulum, as affected by both components, or the whole, of the rotation of the earth (see NOTE G)—

$$t=\pi\sqrt{\frac{l}{g}}\left\{1+\frac{r\omega^2}{2g}\left(\sin^2\lambda-\cos^2\lambda\cos z\right)\right\} \quad . \; . \quad . \qquad (5)$$

If always made to swing in the meridian, it will gain at the equator at the same rate as a free pendulum so swinging which has the same l and r; and it will lose at the poles at that same rate (though of course the free pendulum will not do so); and at latitude 45° its rate will be unaffected by its rotation with the earth. In general, in order that the knife-edge pendulum should be unaffected by its rotation with the earth, its plane of vibration should have such an azimuth z that $\cos z = \tan^2\lambda$. This relation is, of course, impossible in latitudes higher than 45°; therefore in such latitudes the knife-edge pendulum must always swing, because of its rotation with the earth, more slowly than is due to the length of its radius of oscillation.

We see that two pendulums with the same calculated l, or radius of oscillation, at the same locality, and with parallel planes of oscillation, will not go together with perfect accuracy, on the rotating earth, unless they have also the same r, or mass-radius. If the pendulum be a straight uniform rod, it will have the same l, or calculated radius of oscillation, viz., two thirds of its whole length, whether it be swinging about one end, or about a point of trisection; but its r will be three times as great in the former case as in the latter; and the rate of gaining will also be greater in the same proportion.

We see also that, in consequence of its rotation with the earth, the point of suspension and the actual centre of oscillation of a pendulum are not interchangeable; except under the condition that the centre of mass is halfway between those two points, which, of course, is a quite possible condition.

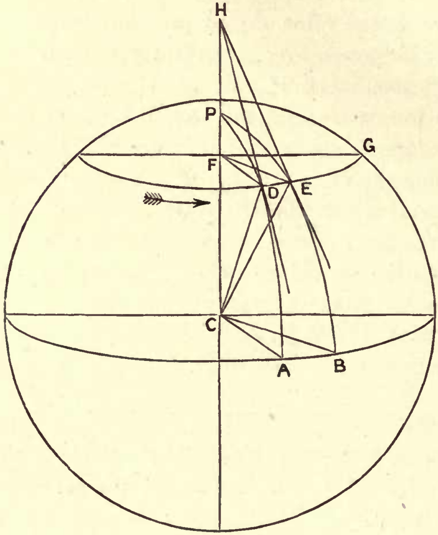
The present matter would be of no practical importance in the ascertainment of the value of g by pendulum experiments. Still it should not be passed over altogether without notice; it ought to be at least mentioned, if only for the purpose of pointedly excluding it from consideration. A difference of one hundredth of an inch in the height of the barometer would be taken account of in obtaining the value of g by the pendulum; and it is by no means self-evident beforehand that the rotation of the instrument with the earth has less effect on its rate of vibration than that apparently quite insignificant item of consideration.

The apparent course of a projectile is affected by both component rotations, V and M. But it will be better to consider this separately in the next chapter.

NOTE A, from p. 12.—In Fig. 1 let the circle be the outline of the earth, P its north pole, and C its centre. Let D be the situation of the place of observation at a certain instant, and PDA the meridian line of said place, DEG being its parallel of latitude. Suppose that the rotation of the earth about its axis PC, in the direction indicated by the arrow, would carry the place of observation from D to E in one second of time. Draw tangents at D and E to the surface of the earth in the meridian planes of those points, meeting the production of the earth's axis in H, and complete the diagram. The angle FDH is evi- FHD
dently the latitude of D, or λ. In one second the earth has turned through the angle ACB, or DFE, or ω. But the horizontal N. and S. line through the place of observation, now at E, has turned only through the angle DHE. Now the angles DHE and DFE, being both exceedingly small and with the same sub-

tense, as we may call it, they are inversely proportional to their D radii HD and ~~H~~F, or directly as $\sin \lambda$ to 1. Therefore in one second the face of the ground at the place of observation has turned in its own plane through $\omega \sin \lambda$.

Fig. 1.



Again, CD and CE produced are vertical lines at D and E. Therefore, in the same time, the vertical line at the place of observation has turned eastwards about the horizontal N. and S. line, as axis, through DCE. Now DCE and ACB, being both exceedingly small and with equal radii, they are to each other directly as their subtenses, or as FD to CA, that is as $\cos \lambda$ to 1. Therefore in one second the vertical line at the place of observation has turned eastwards about the N. and S. horizontal line at that place as axis, through $\omega \cos \lambda$.

NOTE B, from p. 16.—First let us prove the following, to be used again in p. 17. A perfectly free body is moving horizon-

tally, always directly away from its starting-point. Its radius-vector, or the line from that point to itself, will have, in N. lats., a uniform angular velocity of deflection to the right, relatively to the ground beneath, the magnitude of which is $\omega \sin \lambda$. Now let the body have the uniform velocity v along its radius-vector r, so that $r=vt$, t being the length of the time of the movement. The velocity of the linear sideward shifting of the body is $r\omega \sin \lambda$, or $vt\omega \sin \lambda$; it therefore increases uniformly with the ime, that is with a constant acceleration, which we shall call a. The linear space described in the first second of time under this constant acceleration is $v\omega \sin \lambda$. Therefore $a=2v\omega \sin \lambda$, per sec., per sec. Multiplying the right side of this equation by m, the mass of the body, and the left side by the equivalent W/g (W being the weight of the body and g gravity), we have for ma, or the apparent sideward pull on the body, or F,

$$F=\frac{2v\omega \sin \lambda}{g}W.$$

The rightward sidling of the body, relatively to the ground beneath it, is as though it were produced by a constant force or pull F, of the magnitude now given. And if that sideward shifting be stopped by some impediment (such as the right-hand rail in the case of a railway train in N. lats.), the forward v remaining the same, the body will continue to press against the impediment with that force F.

Now if the impediment be that of the resistance of the air, the body's, in this case the pigeon's, sideward motion will at first increase, until the consequently increasing resistance of the air to that motion becomes $=F$. The sideward velocity then becomes uniform, like the bird's forward velocity along the radius-vector.

Let s be the sideward shift in one second when this has taken place. Then s/v is the tangent of the angle between the tangent to the curve and the radius-vector, at any point of the curve. That angle is then constant; and this is a distinguishing property of the logarithmic spiral.

Or thus—$\frac{s}{v}=\frac{rd\theta}{dr}$, as is evident, or $d\theta=\frac{s}{v}\frac{dr}{r}$; whence $\theta=\frac{s}{v}\log r+C$; C being a constant which we do not now want to determine. Thus when the sideward velocity becomes uniform, but not until then, the curve settles into a logarithmic spiral whose pole is at the starting-point.

Now suppose the bird to do the opposite, viz., to fly towards a given point with the velocity v, always turning so as to keep his head directly towards the point, notwithstanding his continual shifting rightwards from the rotation of the earth. It is easily seen that he will describe a similar spiral backwards; the pole being at the goal-point. In Fig. 2, Aa and bc are intended

Fig. 2.

to represent v, ab and cd to represent s. As in p. 16, A is not the pigeon's starting-point on his homeward flight; but the point at which his sideward shifting has become constant. For clearness this figure and the next have been drawn altogether out of scale. L is the light-house.

The equation $\theta=\frac{s}{v}\log r+C$, though perfectly accurate if the problem, as stated, be regarded as one of abstract kinematics, will, for certain obvious dynamical reasons, not be realizable in the concrete case of the pigeon for distances too near the pole. If the logarithmic spiral A bd were produced backwards towards the pole L, it would make an infinite number of turns round the pole before reaching it; which, in accordance with the statement of the problem, would have to be described by the bird in a very short time. Near the pole the bird could not, and would not if he could, do as we have proposed for him. But for the distances therefrom with which we are now concerned we cannot doubt

that he would do so; and his departure from the logarithmic spiral due to his inertia (for there would be such) would be quite insensible.

Let us assume that the resistance of the air to the transverse velocity is proportional to the square of that velocity, and therefore to s^2. The resistance due to the final transverse velocity being, as we have called it, F, $s = K\sqrt{\mathrm{F}}$; in which K is a constant. We know the value of F from the above; that of K can be ascertained only by experiment*. It would appear that it is just about 9·8, if the weight of the bird be expressed in ounces. The approximate correctness of this has received a certain satisfactory confirmation. We have then

$$s = 9{\cdot}8\sqrt{\frac{2v\omega \sin\lambda \mathrm{W}}{g}} \text{ ft., or } 9{\cdot}8\sqrt{\frac{2\times 40\times \sin 51^\circ\, 30'\times 14}{13713\times 32{\cdot}2}} \text{ ft.,}$$

which is 0·4366 ft.; and s/v, the tangent of the tangential angle, is 0·0109, or 1/92, very nearly. We neglect the quite unimportant effect of the difference of latitude between A and the light-house.

The greatest departure of the pigeon to the right of AL is easily obtained very approximately in this case. Since s/v is so very small, it differs very slightly indeed from the circular measure of the angle 38′, of which it is the tangent, and also from the sine of that angle. If DB, Fig. 3, be the greatest distance of the curve from LA, the tangent at D is parallel to LA, and DLA is equal to what we have called the tangential angle of the curve. DB, which we wish to ascertain, is LB$\frac{s}{v}$; or (as DLA is so very small) LD $\frac{s}{v}$, *quam prox.* Now

* Experiments were made with a falling inverted cone of light paper estimated as presenting to the air through which it moved a horizontal areal section *equivalent* (not equal) to that of the side aspect of a flying homer. The coefficient K is the number of feet fallen through by the cone in one second, after attaining its final velocity, divided by the square root of the number of ounces in its weight.

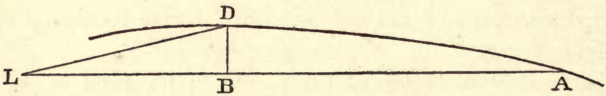
let θ be the angle between LA and the selected axis or prime vector, wherever that may be, and θ' the angle between LD and the same; then we have $\theta = \frac{s}{v} \log \mathrm{LA} + K$, and $\theta' = \frac{s}{v} \log \mathrm{LD} + K$.

Therefore $\theta - \theta'$, or angle DLA, or *q. pr.*, $\frac{s}{v}$

$$= \frac{s}{v}(\log \mathrm{LA} - \log \mathrm{LD}) = \frac{s}{v} \log \frac{\mathrm{LA}}{\mathrm{LD}},$$

thus $\frac{s}{v} = \frac{s}{v} \log \frac{\mathrm{LA}}{\mathrm{LD}}$; whence $\log \frac{\mathrm{LA}}{\mathrm{LD}} = 1$, which is the logarithm of the base of the system of logarithms, viz.: the Naperian. Thus LA/LD = that base; and LD is 10 miles /2·71828, or 3·68 miles, and DB is this $\times \frac{s}{v}$ (*i. e.* by $\frac{1}{92}$) which is 70 yards, very nearly.

Fig. 3.

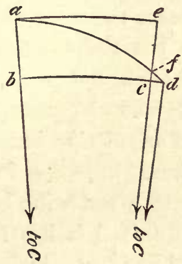


NOTE C, from p. 19.—Though the following geometrical proof of this, by R. A. Proctor, is on the same lines as that given in Chapt. III., NOTE C, for another deviation, we may consider it here on account of the use to be made of it in the next NOTE to this.

In Fig. 4, let *bcd* be the surface of the earth and C its centre, and let *ab* be the height of the fall. The body, ready to drop from *a*, has been describing the continuation of *ea* beyond *a*, with a uniform areal velocity about C. When let go it describes the (absolute) curve *ad* under the force of gravitation directed to C, and therefore with the same areal velocity about C as it had before. The curve *ad*, though really an ellipse with the centre of the earth in one focus, is sensibly a parabola. Suppose that when the body has reached *d*, the top of the height of fall has

reached e; draw eC. We can see quite easily, *à priori*, that cd and cf are so exceedingly small, relatively to ab and bc, that the proportional difference between ec and ef may be neglected with-

Fig. 4.



out sensible inaccuracy. Now the areas aCe and aCd are equal, as describable in the same time; and therefore taking away the part common to both, aef is equal to fCd. Then, since $abce$ is sensibly a rectangle, and, as we have said, ec may be taken for ef without appreciable error, we have, from a well-known property of the parabola, $\frac{1}{3}ab \times bc = \frac{1}{2}R \times cd$; or $\frac{1}{3}hR\omega \cos \lambda t = \frac{1}{2}R\delta$; R being the earth's radius. That is to say, $\delta = \frac{2}{3}ht\omega \cos \lambda$. Q.E.D.

Note D, from p. 20.—This Guglielmini must not be confounded with the distinguished physicist, with the same surname, also of Bologna, who died in the year 1710. He described his above experiments in a work *De motu terræ diurno*, Bologna, 1792, quoted by Delambre in *Astron. Theor. et Prat.* tom. ii. p. 192. Benzenberg described his experiments in a book *Versuche über das Gesetz des Falles*, Dortmund, 1804, and in *Versuche über die Umdrehung der Erde neu berechnet*, Düsseldorff, 1845. For an account of Reich's *Fallversuche über die Umdrehung der Erde*, see Poggendorff's *Annalen*, vol. xxix. 1833, p. 494, as also Houel's *De deviatione meridionali corporum libere cadentium*,

Utrecht, 1839. This experiment has been tried also at Verviers in Belgium, and doubtless elsewhere.

NOTE E, from page 20.—This can be readily seen thus. In Fig. 4 the curve ad is sensibly a parabola; but now as the velocity of descent is uniform, ad is sensibly a right line (but of course much longer than before for the same ab). The area aCd is still equal to aCe; because the resistance of the air on which it depends is sensibly (though not accurately) a central force, though directed from C; and aef, which we have agreed to take as aec, is now one half of $ab \times bc$, instead of one third of it; consequently the equation (2) becomes $\delta = ht\omega \cos \lambda$. Q.E.D.

Of course the parachute, after being let go, will not attain its final and constant velocity until it has fallen a short distance; in the present case about one foot. This will make the resulting deviation less than what is given in formula (3), just demonstrated; but, for a fall of 80 feet, or more, the difference is so small, proportionally, as to be quite unimportant.

NOTE F, from p. 22.—The absolute centrifugal force being, as we have said, $r\omega^2 \cos^2\lambda$, if the plane of vibration be inclined to that of the meridian at the azimuth angle z, the effective part of the c.f. will be $r\omega^2 \cos^2 \lambda \cos z$, and the pendulum will oscillate under $g + r\omega^2 \cos^2 \lambda \cos z$, acting at the centre of mass and parallel to g. Therefore the time of vibration of the free pendulum is not $\pi\sqrt{\frac{l}{g}}$, but

$$\pi\sqrt{\frac{l}{g + r^2\omega^2 \cos^2 \lambda \cos z}};$$

which can be written, *quam prox.*, as equation (4) in text.

NOTE G, from p. 24.—It is easy to see that for very small amplitudes of oscillation the tangential component of the c. f., now in question, acting on the centre of mass away from the position of rest of the pendulum, is $r\omega^2 \sin^2 \lambda \sin \theta$. This then acts at the same point, and according to the same law of distance

from the point of rest, as the tangential component of gravity, or $g \sin \theta$. Therefore, while in p. 22, and in NOTE F, $r\omega^2 \cos^2 \lambda \cos z$ had to be added to g, now $r\omega^2 \sin^2 \lambda$ must be subtracted from their sum, making $g + r\omega^2(\cos^2 \lambda \cos z - \sin^2 \lambda)$. Therefore the time of vibration of the knife-edge pendulum, as affected by its rotation with both V and M, is

$$\pi\sqrt{\frac{l}{g + r\omega^2(\cos^2 \lambda \cos z - \sin^2 \lambda)}},$$

which can be written, *quam prox.*, as equation (5) in text.

CHAPTER III.

DEVIATION OF PROJECTILES FROM THE ROTATION OF THE EARTH.

THIS interesting subject, though coming under the heading of the last chapter, seems worthy of having a chapter to itself. It is treated imperfectly in elementary books, &c., which cannot afford to give it the amount of space that could be desired. A sometimes important factor of the question, viz., the westward shift of the point of fall of the projectile from the earth's rotation, is usually overlooked; and this sometimes gives occasion to certain incorrect statements (see footnote, p. 42); besides which, in the works just referred to the alteration of the range of the projectile's flight by the rotation of the earth is neglected altogether. (See NOTE A.)

The present subject, though a very interesting one in itself, is of but little *practical* importance. The effects with which we are now concerned are so overborne and masked by other disturbances of accuracy in the intended flight of projectiles, that they may be not even mentioned in a modern text-book of gunnery. They are, however, recognized by the Royal Artillery Institution.

It is hardly necessary to observe that the deviation now in question is, unlike the others, only apparent, and relative to us; like the rising and setting of the sun. It is not the projectile which departs from its course in a certain direction, but the earth which turns beneath it in the opposite direction.

The principle concerned in the deviation of projectiles from

the rotation of the earth depends on the existence of the two components of the earth's angular movement of rotation, which we have considered in Chapter II. The component of the earth's rotation which has the vertical line at the place of discharge as its axis we have called component rotation V, its angular velocity being $\omega \sin \lambda$; that which has the horizontal meridian line at the place of discharge as its axis, we have called component rotation M, its angular velocity being $\omega \cos \lambda$. We shall now consider the apparent effects of these separately on the projectile's motion.

The net effects on the projectile's motion consist of alteration of range and lateral deflection; but these do not correspond, respectively, to the two causes just mentioned. The orderly arrangement of this subject presents, therefore, a slight difficulty. The simplest and most convenient division seems to be that presented in the following summary.

N.B. The resistance of the air is provisionally disregarded; but we shall consider further on how it affects the applicability of the following formulæ.

Summary.—The shift of the point of fall of the projectile from what it would be for a non-rotating earth is compounded of *three shifts* (*a*), (*b*), and (*c*), which can be considered and calculated separately, viz.:—

(*a*) *The (purely) Longitudinal Shift.* This is directed along the line of projection. The alteration of range is an increase, or a decrease, respectively, according as the direction of firing has in it any easting or westing. Therefore, except in firing due S. or N., when it is zero, it always has an eastward tendency. Other things being equal, this varies as the sine of the azimuth of projection. Like (*b*), it is proportional to the range and to the time of flight (but it depends also on the angle of the projectile's descent). Like (*c*), it is due to the earth's component rotation M.

(*b*) *The* (*purely*) *Transverse Shift* at right angles to the line of projection. It is a deflection to the right hand in N., and to the left in S. latitudes. Other things being equal, this is the same for all azimuths, or horizontal directions, of projection. It is proportional to the range and to the time of flight. It is due to the earth's component rotation V.

(*c*) *The Westward Shift.* This is directed due W., both in N. and in S. latitudes. Other things being equal, this is the same for all azimuths of projection. It is proportional to the height of the trajectory and to the time of flight. For firing N. or S., this is, of course, wholly a transverse shift or deflection; for firing E. or W., it is wholly a longitudinal shift, or alteration of range. But of course, in general, this shift is both a deflection and an alteration of range. The alteration of range involved in it, whether increase or decrease, is always opposite to the purely longitudinal shift (*a*). The deflection involved in it is to be added to, or subtracted from (*b*), according to circumstances. This shift, like (*a*), is due to the earth's component rotation M; but it depends thereon in a totally different manner; being connected with the height, not the range, of the trajectory.

The net result is a whole longitudinal shift of the point of fall of the projectile, or alteration of range, which is (*a*) modified by one resolved part of (*c*); and a whole transverse shift, or deflection, which is (*b*) modified by the other resolved part of (*c*).

We now proceed to the demonstration of the above. It should be remembered that the following calculations are only approximately correct, even for a vacuum. Certain quantities of higher orders than the first are neglected; but the result of this is practically insensible; as the ranges attainable by actual ordnance are so very small in proportion to the dimensions of the earth, and, moreover, as the longest time of flight of any actual projectile is so very small compared with the period of the earth's rotation.

(*a*) *The* (*purely*) *Longitudinal Shift.*—This shift along the line of projection constitutes an alteration of range, which will be, both in N. and in S. latitudes, an increase, if the direction of projection have in it any easting, and a decrease if any westing. The question of this shift, as it presents itself to us, is simply a kinematical one.

We shall begin the consideration of this with the case of firing E. It is evident that if the surface of the ground at the place of discharge were moving straight on in its own plane, its motion would cause no difference in the range, on the earth's surface, of the trajectory. But the surface of the ground at the locality of firing is being always tilted over towards the east, with the angular velocity $\omega \cos \lambda$, whilst being translated in that direction. Whilst the projectile is flying, as now supposed, towards the east, the ground beneath it is turning away from it downwards, if we may so express it; so that the projectile will pass above the point on the surface of the ground on which it would have fallen for a non-rotating earth; and it will not reach the ground until it has gone some distance beyond that point. The opposite of this takes place, of course, in the case of firing W.* Thus this shift of the point of fall is due east, both for E. and for W. firing; and it has an eastward tendency for all azimuths of discharge, except N. and S.

It is quite easily seen (Note B) that the magnitude of *this* alteration of range for E., and for W., firing is

$$rt \cot \delta \, \omega \cos \lambda \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which r is the length of range, t the time of flight in seconds, δ the angle of *descent* at the end of the trajectory, ω the earth's angular velocity of rotation about its axis, or angle described per second (which, as we have seen in Chapt. II. is

* It is evident that there is also an increase or a decrease, respectively, in the height of the trajectory for E. and for W. firing, and an accompanying increase or decrease in the height at which the ball would hit a target.

represented in circular measure by the fraction 1/13713), and λ the latitude of the place of discharge.

For any azimuth of discharge, z *, this must be multiplied by $\sin z$; so that in general this alteration of range is

$$rt \cot \delta \, \omega \cos \lambda \sin z; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

an increase, if there be any easting in the direction of discharge, with $\sin z$ positive; a decrease, if there be any westing, with $\sin z$ negative. This is applicable both to N. and to S. latitudes.

(*b*) *The* (*purely*) *Transverse Shift*.—This, as we have said, is a shift of the point of fall of the projectile, from what it would be for a non-rotating earth, at right angles to the line of projection. It is directed to the right hand in N., and to the left in S., latitudes.

Considering for the moment only the earth's component rotation V, to which this shift is due: if a projectile were discharged towards some suitable object standing on the ground, that is, discharged in the plane passing vertically at the instant through that object, it would continue to move in that plane. But in consequence of the turning of the surface of the ground in its own plane, with the angular velocity $\omega \sin \lambda$, the object aimed at would pass, in N. latitudes, to the left of the vertical plane of discharge; leaving the projectile behind to the right of it. It is evident that the rate of *this* apparent angular deviation of the projectile to the right, or the angle described in one second, being due to this cause alone, must be the same for all azimuths, or horizontal directions, of discharge, and equal to $\omega \sin \lambda$. The angle described during the time of flight, t seconds, is $t \omega \sin \lambda$; and to get the linear shift of the point of fall of the projectile, from what it would be on a non-rotating earth, at the end of t, we must multiply this by the range r. Now this shift, as is evident, does not involve any alteration in the length of the range; it is simply an apparent linear *deflection* from the line of

* We now reckon the azimuth from S. eastwards, and continuously right round the horizon.

discharge. It is very easily seen that the expression for this (purely) transverse shift (to the right in N., and left in S. latitudes), neglecting the resistance of the air, is

$$rt\,\omega \sin \lambda. \qquad (3)$$

Let us observe that the question of this deflection is, like that of (*a*), merely a kinematical one, relating only to angular and linear motion; it differs, in this respect, from the question of the westward shift, which, as we shall see, is a dynamical one. Let us observe also that the above evaluation of the (purely) transverse shift rests simply upon the fact that the moving body accomplishes the distance r, in the time t, quite irrespectively of the law of its velocity in its flight.

(*c*) *The Westward Shift.*—This is a shift due W., both in N. and in S. latitudes, of the point of fall of the projectile, from what it would be for a non-rotating earth. The present question, unlike that of the (purely) transverse shift and the (purely) longitudinal shift, is, as we have said, a dynamical one.

Still supposing the projectile to move in a vacuum, we shall consider first the case of a shot fired vertically.

During its flight the locality of discharge has not been simply translated towards the E. by the rotation of the earth (if this were so, those would be right who say that a bullet fired vertically will fall on the muzzle of the gun); but, as already mentioned, it has also been tilted over somewhat towards the E. The vertical line of the place has moved angularly towards the E., so that the projectile is left behind by it towards the W., both in N. and in S. latitudes; just as it is left behind towards the right in consequence of the horizontal component, at the place, of the earth's rotation. But by the time the projectile has returned to the earth its westward falling-behind from the vertical line will have increased. It is evident that the magnitude of this shift is connected with the greatest height to which the projectile attains, as well as with the time of flight.

The amount of this westward shift in a vacuum, for vertical

firing, is $\frac{4}{3}\,ht\,\omega\cos\lambda$; h being the greatest height attained by the projectile. This results from the principle of the equable description, by the projectile, of areas about the centre of the earth, or Kepler's Second Law, and the fact that the area included by the sensibly parabolic (absolute) trajectory and the (level) ground is two thirds of the rectangle under base and height of trajectory. For proof see NOTE C.

Now it is evident that there must be always, for any angular elevation of discharge, as well as for vertical firing, such an action as this connected with the vertical component of a projectile's motion, and that the westward deviation, or shift, of the place of fall of the projectile, must be the same as for vertical firing, if h and t be the same, and that, *cæteris paribus*, it must be the same for all azimuths of firing. Therefore the amount of this shift due W., for any trajectory with given h, is the same as that mentioned above for vertical firing. It is

$$\frac{4}{3}\,ht\,\omega\cos\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

As a general rule, this involves both an alteration of range and a deflection. The alteration of range is compounded with shift (a), treated above; the deflection with shift (b).

As to the alteration of range involved in this westward shift, it is this shift multiplied by $\sin z$ (see footnote, p. 38); therefore *this* alteration of range is

$$\frac{4}{3}\,ht\,\omega\cos\lambda\sin z\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which is a decrease of range if the direction of discharge have in it any easting, and an increase if any westing. There is, of course, no change of range for S. and for N. firing. All this is applicable both to N. and to S. latitudes.

As to the deflection involved, it is, of course, this westward shift multiplied by $\cos z$; therefore *this* deflection is

$$\frac{4}{3}\,ht\,\omega\cos\lambda\cos z. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This deflection, as is evident, is to the right, whenever the

direction of discharge has in it any southing, and to the left when any northing; it is zero for E. and for W. firing. All this being applicable both to N. and to S. latitudes.

Net Results.—The *whole resulting longitudinal shift*, or alteration of range, for any azimuth of discharge z, whether the latitude be N. or S., is (2) *minus* (5), or

$$t\,\omega \cos\lambda\,(r \cot\delta - \tfrac{4}{3}h)\sin z. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

If δ be small enough, as it is in all ordinary trajectories (whose angles of elevation never exceed 45°), $r \cot \delta$ will be greater than $\frac{4}{3}h$; and if there be any easting in the direction of discharge, which would make $\sin z$ positive, the alteration of range will be an increase; and if there be any westing in that direction, making $\sin z$ negative, a decrease; and *vice versâ*, if δ be great enough to make $r \cot \delta$ less than $\frac{4}{3}h$; which last would imply a very high angle of elevation, such as is never in practical use. If these two quantities be equal, there will be no alteration of range for any azimuth of projection. To make them equal, the angle of elevation of discharge must be, in a parabolic trajectory, 60° (see NOTE F); but in a ballistic trajectory that angle must be less than 60°; how much less depends on circumstances. The factor $\sin z$ shows, what indeed is evident beforehand, that in any case, for firing due S. or N., there will be no alteration of range; and that for firing due E. or W. the alteration is a maximum, whether positive or negative. Both parts of this shift are due to M.

Again: The *whole resulting transverse shift*, or deflection, of the point of fall of the projectile, for any azimuth of discharge z, is the algebraical sum of (3) and (6) taken with their proper signs; that is

$$t\,\omega(r \sin\lambda + \tfrac{4}{3}h \cos\lambda \cos z). \quad . \quad . \quad . \quad . \quad (8)$$

This total deflection consists, then, of two parts; one being due to the earth's component rotation V and proportional to the range r; the other being due to the earth's component rotation

M and, for given z, proportional to the height h of the trajectory.

Taking for example the case of N. lats.—If the direction of discharge have in it any southing, $\cos z$ is positive, and we see, what indeed is evident beforehand, that the whole actual deflection is the sum of the two, and a maximum for firing due S. If the direction of discharge have in it any northing, $\cos z$ is negative, and the whole deflection is then the difference of the two; and, if $\cos z$ be equal to $-\frac{3r}{4h}\tan\lambda$, the whole deflection will be zero. If $\cos z$ be greater than $-\frac{3r}{4h}\tan\lambda$, as it may easily be with a combination of great enough h, sufficient northing of discharge, and low enough latitude, the whole deflection will be to the left; though the latitude be N.* (See Figs. 5 and 6.) Correspondingly, *mutatis mutandis*, for south latitudes.

It may be of interest to observe that while the purely longitudinal shift can never exist alone, the purely transverse shift would exist alone for firing from the N. or the S. pole, and the westward shift would be the only one for firing from the equator either due N. or S.

The Resistance of the Air as affecting the above Formulæ.—So far we have disregarded the resistance of the air to the motion

* It is often stated in elementary books, &c., that the deflection of a projectile from the rotation of the earth is to the right in N., and to the left in S., latitudes, and, for a given trajectory, the same for all azimuths of discharge, and that there is no deflection if the projectile be discharged from the equator. But this is to consider only the (purely) transverse shift (b), formula (3), in disregard of the transverse component of the westward shift, formula (6). However, it is true that for ordinary (that is, somewhat flattish) trajectories in middle and higher latitudes the actual whole deflection is to the right in N., and to the left in S., lats.; but it is by no means the same for all azimuths of projection (see table in p. 46).

of the projectile while discussing the three shifts of the point of its fall due to the rotation of the earth. But we shall find that, although the resistance of the air has such a great influence on the motion of projectiles, diminishing the range, and making the trajectories to be ballistic instead of parabolic ones, yet it affects but very little the applicability of our above formulæ.

The reason of this is that those formulæ are expressed in terms of those elements of the trajectory on which the shifts *directly* depend; viz., the range, the height of the apex, the angle of *descent*, and the time of flight. The shifts, as we have seen, do not depend on any relations (whether parabolic or ballistic) of those elements to each other; it is only the magnitudes of the specified elements which are concerned, whether they have been attained with or without the resistance of the air.

With respect to formula (3) for the (purely) transverse shift, it is, as we have already said, independent of the law of the horizontal motion of the projectile. The horizontal component of the resistance of the air to that motion does not affect, in the slightest degree, the validity of that formula, which is concerned only with the easily observed magnitude of the range and that of the time of flight, without any reference to the law of the velocity under which the range has been attained.

With respect to formula (1), for the (purely) longitudinal shift, the same remark applies to the range, as it occurs therein; and as δ is the actual angle of descent, relative to the spectator, at the instant of the fall of the projectile, formula (1) needs no modification for the resistance of the air to the projectile's own forward motion.

With respect to formulæ (4), for the westward shift, and (5), for its component deflection, and (6), for its component alteration of range, which all depend upon h, their applicability is hardly affected by the vertical component of the resistance of the air to the projectile's motion (see NOTE D).

But if the validity of the above formulæ is thus practically

uninfluenced by the resistance of the air to the projectile's own *proper* forward motion, how is it with respect to the transverse resistance of the air to the projectile's apparent motion of deviation due to the earth's rotation? It is evident, at once, that this must cause a diminution of the shifts, and also that, considering the high densities of the projectiles with which we are concerned, this diminution must be quite small. It can be easily calculated, from empirical data bearing on the subject, that the greatest deviation in the following long-range tables has to be diminished, on this account, only by considerably less than one hundredth part, and that the other (smaller) deviations in those tables are to be diminished in still smaller respective proportions. We may now, therefore, neglect this particular altogether.

For parabolic trajectories in a vacuum the above formulæ could be readily expressed in terms of the initial velocity of the discharge, its angle of elevation, and g, by means of the familiar equations for such trajectories. But in *that* shape they would be altogether unsuitable for ballistic trajectories in resisting air, as they would involve the special relations to each other of the elements of parabolic trajectories *.

In illustration of the above, it will probably be most interesting to select an extreme example, suggested by the "Jubilee Rounds" fired at Shoeburyness in April and July, 1888, in celebration of the 50th Anniversary of the Queen's Accession to

* We may mention here that all the foregoing formulæ, arrived at geometrically, are in accordance with the results of Professor Bartholomew Price's analytical discussion of the same subject (excepting a certain *lapsus calami*) in his *Infinitesimal Calculus*, 2nd ed., 1889, vol. iv., though he has not explicitly separated the different parts of the question. His expressions are intended for parabolic trajectories *in vacuo*; they are in terms of the initial velocity, the elevation of the discharge, and g, and involve the principles of such trajectories; they are, therefore, inapplicable to ballistic ones. (See NOTE E.)

the Throne. See the paper by Lieut. A. H. Wolley-Dod, R.A., in the Minutes of *Proceedings of the Royal Artillery Institution*, vol. xvi., p. 491, also Bashforth's *Revised Account of Experiments made with the Bashforth Chronograph*, 1890, p. 114, &c., also the London *Times*, Sept. 25th, 1890.

The cannon used was a 9·2 in. wire breech-loading gun, weighing 22 tons; the charge 270 lb. of powder; the shot an ogival-headed bolt with diameter 9·2 in., length about 28·5 in., and weight 380 lb.; the muzzle velocity 2360 ft. per sec. (or a little more). On July 26, with the elevation of 45°, the greatest range was attained; viz., the enormous one of 21,800 yds., or nearly 12·4 miles; but this was with the assistance of a "favourable moderate" wind. Prof. Bashforth calculated that the range in still air would have been 19,944 yds., or 11·33 miles. Though such calculations profess to be only approximate, yet, as Lieut. Wolley-Dod observes, "It seems to have been amply proved that, even at extreme ranges, the formulæ and tables will give correct results."

We shall now adopt the trajectory as calculated by Bashforth; it being the last one given by him in p. 116 of his work referred to above. His calculation has been made for a horizontal plane 27 ft. below the muzzle of the gun; but the effect of this on the deviations may be called quite insensible.

The comparative smallness of the alterations of range is due to the greatness of the angle of elevation of discharge, involving a relatively large h and a high angle of descent; in consequence of which the two oppositely-directed elements of alteration of range (formula 7) are beginning to approach equality.

Range 19,944 yds. (11·33 miles); height of apex of trajectory 19,648 ft. (3·72 miles) time of flight 68·3 secs.; angle of descent 58° 43′.

For lat. 51° 31′ N.

Azimuth of discharge.	Deflections in yards. *form.* (3) + *form.* (6) = *form.* (8)	Changes of range in yards. *form.* (7)
S.	$77{\cdot}78 + 27{\cdot}07 \cos 0° = +104{\cdot}85$ to right.	$10{\cdot}5 \sin 0° = 0{\cdot}00$
S.E.	$77{\cdot}78 + 27{\cdot}07 \cos 45° = +\ 96{\cdot}92$ do.	$10{\cdot}5 \sin 45° = +\ 7{\cdot}43$ incr.
E.	$77{\cdot}78 + 27{\cdot}07 \cos 90° = +\ 77{\cdot}78$ do.	$10{\cdot}5 \sin 90° = +10{\cdot}50$ do.
N.E.	$77{\cdot}78 + 27{\cdot}07 \cos 135° = +\ 58{\cdot}64$ do.	$10{\cdot}5 \sin 135° = +\ 7{\cdot}43$ do.
N.	$77{\cdot}78 + 27{\cdot}07 \cos 180° = +\ 50{\cdot}71$ do.	$10{\cdot}5 \sin 180° = 0{\cdot}00$
N.W.	$77{\cdot}78 + 27{\cdot}07 \cos 225° = +\ 58{\cdot}64$ do.	$10{\cdot}5 \sin 225° = -\ 7{\cdot}43$ decr.
W.	$77{\cdot}78 + 27{\cdot}07 \cos 270° = +\ 77{\cdot}78$ do.	$10{\cdot}5 \sin 270° = -10{\cdot}50$ do.
S.W.	$77{\cdot}78 + 27{\cdot}07 \cos 315° = +\ 96{\cdot}92$ do.	$10{\cdot}5 \sin 315° = -\ 7{\cdot}43$ do.
S.	$77{\cdot}78 + 27{\cdot}07 \cos 360° = +104{\cdot}85$ do.	$10{\cdot}5 \sin 360° = 0{\cdot}00$

For the same Trajectory. At the Equator.

Azimuth of discharge.	Deflections in yards.	Changes of range in yards.
	form. (6)	*form.* (7)
S.	43·50 cos 0° = + 43·50 to right.	16·87 sin 0° = 0·00
S.E.	43·50 cos 45° = + 30·76 do.	16·87 sin 45° = + 11·93 incr.
E.	43·50 cos 90° = 0·00	16·87 sin 90° = + 16·87 do.
N.E.	43·50 cos 135° = − 30·76 to left.	16·87 sin 135° = + 11·93 do.
N.	43·50 cos 180° = − 43·50 do.	16·87 sin 180° = 0·00
N.W.	43·50 cos 225° = − 30·76 do.	16·87 sin 225° = − 11·93 decr.
W.	43·50 cos 270° = 0·00	16·87 sin 270° = − 16·87 do.
S.W.	43·50 cos 315° = + 30·76 to right.	16·87 sin 315° = − 11·93 do.
S.	43·50 cos 360° = + 43·50 do.	16·87 sin 360° = 0·00

We may now give a diagrammatic illustration, Figs. 5, 6, 7, 8, for diverse azimuths of discharge, but with the same trajectory in all four cases, viz., that selected for the above tables. But lat. 15° N. is now selected in order that the three shifts may, for convenience, not differ too much in magnitude. We could, of course, take the shifts of the point of fall of the projectile in any order we please; but it will be convenient to begin, as above, with (*a*), the purely longitudinal shift. The principle of construction is the same, and the lettering correspondent in all four figs. The thick line *lm* is the latter part of the range for a non-rotating earth; *m* being the end thereof. The dotted line *mn* is the purely longitudinal shift, whether an increase or a decrease of range. The dotted line *no* is the purely transverse shift, to the right, the lat. being N. The dotted line *op* is the westward shift. And the double line *mp* is the whole shift compounded of the others. The letters *l*, *m*, *n*, *o*, *p*, taken in alphabetical order, enable the reader to compare these four diagrams at a glance.

From *m* draw *me* due east, whether the lat. be N. or S., its length representing the value given by formula (1), which is, in this case, 58·3 yards (this *me* is the purely longitudinal shift for E. and for W. firing); from *e* draw *en* at right angles to above range; *mn* is the (purely) longitudinal shift, its value being 58·3 yds. $\times$ sin *z*, formula (2). From *n* draw *no* in the line of *en*, that is at right angles to the range, and towards the right hand in N. lats., and towards the left in S. lats., its length representing the value given by formula (3); this is the (purely) transverse shift, the magnitude of which is, in this case, 25·7 yds. From *o* draw *op* due west, whether the latitude be N. or S., its length representing the value given by formula (4); this is the westward shift, the magnitude of which is, in this case, 42·0 yds. Then the double line *mp* represents, in magnitude and direction, the whole shift of the point of fall of the projectile compounded of the three shifts just mentioned. The whole, or net, longitudinal shift is sensibly the orthographic projection of *mp* on *mn*; and the whole or net transverse shift is the distance of *p*

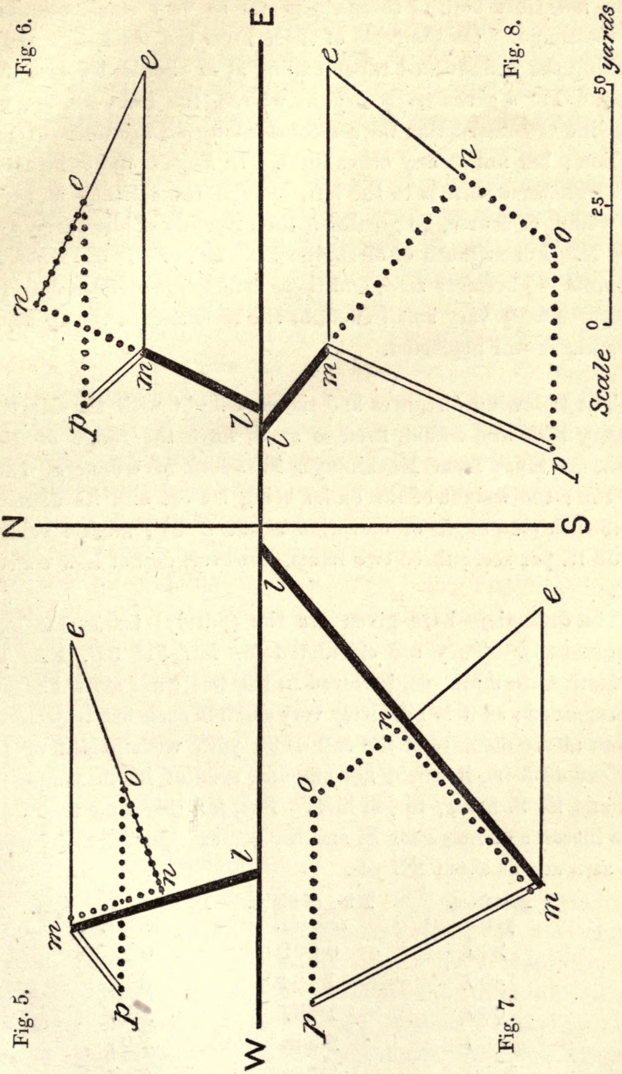


Fig. 5. Fig. 6. Fig. 7. Fig. 8.

from *mn*, since both of these shifts are so very small relatively to the range. On the scale of these Figs. the thick line representing the undisturbed range ending at *m* should be 41·5 feet long. For a given trajectory, as we see, the lines *no*, *op*, and the line of construction *me* are constant for all azimuths of projection; but not so any other lines. In Figs. 5 and 6 the actual net transverse shift is to the left, though the latitude is north. It would, of course, be greater if the direction of discharge were due N. For azimuth of discharge 142° 10′, or 37° 50′ E. of N., formula (8) becomes zero, and there is no lateral deflection. The proportionally very small effect of the resistance of the air on our formulæ is still neglected.

The following distances and times of flight with the Martini-Henry Rifle and Bullet, fired so as to have the range of 1000 yds., are taken from Mackinlay's *Text-book of Gunnery*, 1887, p. 159; the weight of the bullet being 1·1 oz. and its diameter 0·45 inch (the angle of elevation about 2° 31′, muzzle velocity 1353 ft. per sec.; these two items, however, do not now concern us).

The deflections here given are the (purely) transverse ones, formula (3). They are calculated for lat. 51° 31′, N. The deflection, formula (6), involved in the westward shift, has been disregarded; as it is relatively very small in such flat trajectories. Even at the distance of the full 1000 yds., with height of trajectory 45·5 ft., it would not amount, even at its maximum for N. and for S. firing, to $\frac{1}{9}$th inch. But, for this same trajectory, the increase of range for E. and the decrease for W., firing would be as much as about 2·3 yds.

Distance. yards.	Time of flight. seconds.	Deflections, to right. inches.
200	0·501	0·21
400	1·104	0·91
600	1·787	2·20
800	2·548	4·20
1000	3·395	6·96

The diminution of these deflections from the lateral resistance of the air is evidently exceedingly small. It would appear that in the last deflection, where it is greatest, it would only be able to diminish by 1 the digit in the second place of decimals.

NOTE A, from p. 34.—Aristotle contemplated a connection between the earth's rotation, if it existed, and the movement of certain projectiles. He argued (*De Cœlo*, II, 14, 6) that since, as he believed, a heavy body projected vertically upwards falls back on the point of discharge, the earth must be without rotation. He gives no hint of what the effect of the earth's rotation would be in this case, if it existed. But Ptolemy contended (*Almagest*, I, 7) that if the earth rotated with the enormous eastward linear velocity of its surface involved (except, of course, in very high latitudes) in a globe of its size turning completely round in one day, flying birds and projectiles could never get eastward of their point of departure, but would be left a long way behind to the westward of that point.

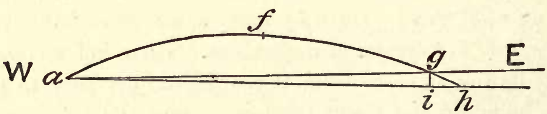
It was reserved for Galileo to give the now so obvious refutation of this objection of Ptolemy's, which he does in his *Systema Cosmicum*. Galileo, however, seems to have considered the connection between the motion of projectiles and the rotation of the earth, not for its own sake, but merely with the object of removing what was regarded by many as a most serious difficulty in the way of the system of Copernicus. His mind was fixed so strongly on this important object that he did not care to go, as fully as he might and could have done, into the question with which we are now concerned.

In page 225 of the London edition, 1663, when disproving the supposed effect, according to Ptolemy's ideas, of the earth's rotation, if it existed, on the motion of a body dropped from a height, he ignores altogether the real deviation from the vertical that must be produced in the fall of such a body by that rotation; and in page 239 he categorically and distinctly declares that a cannon ball discharged vertically would fall back on the

mouth of the cannon, notwithstanding the rotation of the earth. Now, as we have said, Aristotle's words, taken as they stand, mean only that the earth's rotation would prevent a body discharged vertically from falling back on the point of discharge. Thus, then, if we judge them simply by what they say, Aristotle was right and Galileo wrong on this point! But it is greatly to be feared that if we could cross-examine Aristotle and get him to be more explicit, he might commit himself undesirably; and on the other hand, it would not be fair to take Galileo at his word on this point; because we have reason for knowing, from the very work now referred to, that he was better on the present question than he here represents himself to be. His attention was so wholly engrossed with proving that the eastward *translation* of the surface of the earth with everything on it has no effect, relatively to us, on the motion of projectiles &c., that he here disregards the angular *tilting* of that surface towards the east; although he does not do this elsewhere.

NOTE B, from p. 37.—The proof of this is quite simple. Let us first suppose that we are at the equator, and that the discharge is due E. Let a, Fig. 9, be the point of discharge; ag the

Fig. 9.



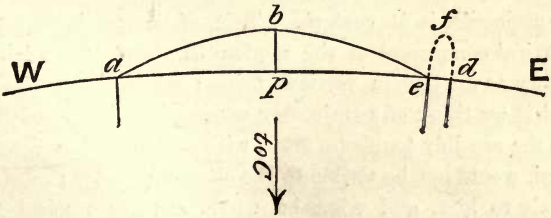
position of the surface of the ground (whose curvature may be now neglected) at the instant of discharge; afg the trajectory, which would intersect the surface at g, if the earth did not rotate; ah the position of the surface of the ground at the instant of fall of the projectile at h. We are now concerned solely with the rotation of the surface of the ground about a horizontal N. and S. axis at a, perpendicular to the plane of the paper. The angle gah is ωt, and very small, even for the longest

attainable time of flight of a projectile. Draw gi perpendicular to ah; ih is the increase of range now under consideration. Now ih is so very small relatively to ag and ah that these two lines may be taken, without sensible error, as having the proportion of equality. Let the angle of descent ghi be δ. Then hi, the increase of range now in question, is $gi \cot \delta$; but gi (as the angle gai is so very small) is $r\omega t$. Therefore (for firing due E. at the equator) $hi = r\omega t \cot \delta$. For azimuth z, we must take, as is evident, $r \sin z$, instead of r; and for latitude λ, we must take, as we know, $\omega \cos \lambda$, instead of ω. Hence for any latitude and azimuth, this alteration of range, hi, $= r\omega t \sin z \cos \lambda \cot \delta$; in which r may be regarded without any sensible proportional error as being ah, the actual range. As with this demonstration, so with the diagram Fig. 9, it is only very approximately correct. The trajectory afh would not rigorously coincide with the supposed one afg, as far as it goes; the former would not be a simple prolongation of the latter, though exceedingly near thereto.

Note C, from p. 40.—The following proof of this (for a vacuum) by Mr. R. A. Proctor, appeared some years ago in the London *English Mechanic*.

First take the case of a projectile discharged vertically at the equator. Let $aped$, Fig. 10, be the surface of the earth, whose

Fig. 10.



curvature and eastward translation must now be recognized. The lines drawn perpendicular thereto at a, e, and d meet at the

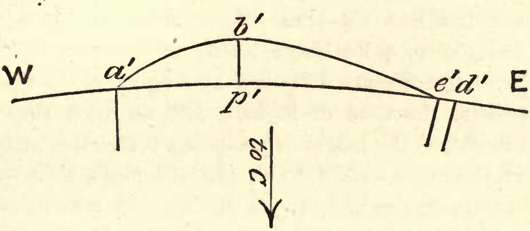
centre, C, of the earth. Let a be the position of the point of discharge at the instant of discharge; abe the orbit described by the projectile about the centre, C, of the earth; bp its greatest height above the surface of the ground; the orbit is an ellipse differing insensibly from a parabola. Let e be the position of the point of fall at the instant of fall; d the position of the point of discharge at the same instant, which will be, as we know, ahead of e. The projectile, having been moving in the backward prolongation of the line ae with a uniform velocity, describing equal areas in equal times about C, has received, at a, an impulse along the radius-vector Ca. If it were quite free it would move uniformly in its new direction of motion, still describing areas about C, per unit of time, equal to the former. But it is acted on by the force of gravity directed to C; this, however, leaves it still describing, about that point, areas the same as before. Therefore the area $aCeb$=area aCd, and area abe = area eCd. That is, from a property of the parabola, $\frac{2}{3}ae \times bp$ * = $\frac{1}{2}R \times ed$; R being earth's radius. But though the *difference*, ed, between ae and ad cannot be ignored, it being the very subject of investigation, yet as it is relatively so exceedingly small, ae and ad have very nearly the *proportion* of equality; so that we can, with very small error, write ad for ae, in our last equation. Hence, very approximately, $\frac{2}{3}ad \times bp = \frac{1}{2}R \times ed$. But as we are at the equator, $ad = R\omega t$; therefore $\frac{2}{3}R\omega th = \frac{1}{2}R \times ed$; and $ed = \frac{4}{3}\omega th$. But, for any other latitude λ, we must evidently use $\omega \cos \lambda$, instead of ω. Hence ed, the westward shift of the point of fall of the projectile, is $\frac{4}{3}\omega \cos \lambda \, th$. This, of course, is as true for the vertical component of the motion of the projectile in any trajectory as for that in vertical firing; which is at once self-evident if we think of a trajectory whose plane is N. and S. Of course the sensibly parabolic orbit, with which we have been now engaged, would not be visible to the observer; the path described relatively to him, and what he would *see*, for vertical firing,

* The inaccuracy introduced by the curvature of the earth's surface into this value of the area abe is quite insensible.

would be like that represented by the dotted curve *dfe*, whose height, of course, is equal to *bp*; the motion of the projectile therein being from *d* by *f* to *e*, or westward, while its motion in the absolute orbit *abe* is eastward.

NOTE D, from p. 43.—This may be seen thus:—Let $a'e'd'$, Fig. 11, be the surface of the earth along the equator. The

Fig. 11.



normals, or lines perpendicular thereto, at a', e', and d' meet at the centre C of the earth. Let the projectile be discharged from a gun pointing vertically at a', in resisting air. Its absolute trajectory will not now be sensibly an upright parabola, as in NOTE C; but something like $a'b'e'$, whose greatest height is $b'p'$. Let d' be the position at which the place of discharge has arrived at the instant of the fall of the projectile. We are now concerned only with the vertical component of the resistance of the air, which is sensibly the same as the whole resistance; the very small difference between them has the effect of diminishing very slightly the westward shift.

Now as the vertical component of the resistance of the air is directed towards the centre C of the gravitation attraction, it does not affect the equable description of areas about C. Therefore (see NOTE B) the area $a'b'e'$ is equal to the area $e'Cd'$; and this is so quite independently of the law of the vertical motion of the projectile.

Now if the curve $a'b'e'$ were a parabola tilted over a little

towards the left, its area would be the same as that of an upright parabola with the same "base," as we may call it, $a'e'$, and height $b'p'$ (with, of course, a greater parameter). But though the curve be not a tilted parabola, it is evident that its area cannot differ much from that of such a parabola.

However, we can easily ascertain, by mechanical means, that its area is sensibly $\frac{2}{3}a'e' \times b'p'$. Let us take, as the least favourable case, the extreme trajectory discussed in the first two tables above, and, selecting a sufficiently large scale, lay down on thick card-board the line $a'e'$ (the *proportional* difference between which and $a'd'$ is quite insignificant) to represent 19·7 miles, which is the linear space described by a point on the equator in 68·3 seconds, the time of flight. Let us draw then a line parallel to $a'e'$, at the height representing 3·72 miles, and having laid down the angle $b'a'e'$ 47° 38′, and the angle $b'e'a'$ 32° 6′ *, sketch in the curve so as to touch the line just mentioned. On cutting out the figure $a'b'e'$ and weighing the piece of card, we shall find that its area is sensibly $\frac{2}{3}a'e' \times b'p'$, or, as in Note B, $\frac{2}{3}a'd' \times b'p'$, very approximately. Whence, as in same place, $e'd' = \frac{4}{3}\omega th$ at the equator, and $\frac{4}{3}\omega \cos\lambda\ th$ at any other latitude λ. This being so with the present extreme height of ascent of the projectile, 3·72 miles, it will be so, *à fortiori*, with smaller heights of ascent, in which the base $a'e'$ (very nearly proportional to t) will have a greater ratio to the height.

The above, as is evident. applies to the greatest height attained by a projectile in any trajectory in air, just as well as if it were discharged vertically.

Note E, from p. 44.—Although the relations among themselves of the respective elements of ballistic and of parabolic trajectories are essentially very different, there is a considerable series of accidental practical exceptions presented to us in Bashforth's table of trajectories in p. 116 of his work referred to above.

* It is easily seen that these two angles result from the data in the last line of the table given by Bashforth in the work above mentioned, p. 116.

With respect to large projectiles, of high specific gravity, describing extensive trajectories, such as we have in that table, it so happens that if a ballistic and a parabolic trajectory have the same t, the respective h's may have quite a small proportional difference. Of course the distribution of t between the ascent and the descent would be very different in the two cases. For the smaller trajectories in that table, the ballistic h is less than the parabolic, with the same t; for the larger trajectories, the ballistic h is greater than the parabolic; and for a considerable intermediate series they are almost equal. Therefore, for such as the last mentioned, the ballistic h in our formula (4) can be replaced by $\frac{1}{8}t^2g$, or $4t^2$, nearly, with a very small proportional error; and formula (8) for the whole transverse shift, which is the most interesting deviation of a projectile, will be approximately correct for such cases, if written

$$t\omega\,(r\sin\lambda + \tfrac{16}{3}\,t^2\cos\lambda\cos z), \quad . \quad . \quad . \quad . \quad (9)$$

which depends only on the easily ascertained elements of the trajectory, r and t.

We may here observe that, for more ordinary, and comparatively flattish, trajectories, in middle and higher latitudes, such as that of London, the h-part in formula (8) is much smaller than the r-part; and therefore, in such cases, whatever proportional error is introduced into the westward shift by substituting therein $4t^2$ for h, it involves a much smaller proportional error in the whole transverse deflection.

Taking these two considerations together, we find that even in the first example in the table in p. 46 above, in which the h-part of the whole deflection is a maximum for that table, the h itself being, moreover, of unusually great proportional magnitude, the error in the whole deflection produced by using the parabolic h, for 68·3 seconds, would not be more than 1·3, out of 104·85, yds.: say $\frac{1}{80}$th part.

An interesting apparent paradox is presented by Bashforth's table of trajectories referred to above, in which the initial velocity is the same in all cases. It is this—that though the

velocity of the projectile at the end of its flight diminishes at first, as we pass from a smaller to a greater range, which we should expect it to do, yet afterwards it does the reverse. That is, after we have passed the range of about 14,000 yards, the greater the distance which has been traversed through resisting air, the greater is the remaining velocity of the projectile at its fall. After we have been informed of this, we can see for ourselves how it may be possible. The initial velocity being given, when the projectile is discharged with a greater elevation, gravity is diminishing its velocity, during its ascent, more rapidly; and therefore, for this reason, by itself, the *average* resistance of the air over the whole trajectory is diminished; and *that* in a higher ratio than the diminution of the average velocity. But, further, the lessening of the resistance is promoted by the circumstance that the middle part of a higher trajectory is described in rarer air. The whole loss of kinetic energy, and of v^2, which has been endured by the projectile when about to fall (the ground being level), is proportional to the *average* resistance multiplied by the length of the curve of the trajectory; and it is very conceivable that under certain circumstances the proportional diminution, which we know to exist, of the first factor of this product might exceed the proportional increase of the second; leaving the v^2, and therefore the v, of the projectile greater after its longer flight. This actually obtains, as regards the series of trajectories now in question, with ranges of 14,000 yards and upwards.

NOTE F, from p. 41.—The following two memoranda, although outside the immediate subject of this Chapter, are appended here at the end of it, on account of their great interest. θ is the angle of elevation of discharge.

(1) In the case of a vacuum and a parabolic trajectory, we could substitute for r, in (7), its value in terms of h, viz. $4\,h/\tan\theta$; thus obtaining, for the whole alteration of range from the rotation of the earth,

$$4\,ht\omega\cos\lambda\left(\frac{1}{\tan^2\theta}-\frac{1}{3}\right)\sin z.$$

This shows that, for a vacuum in any latitude and with any azimuth of discharge, there would be no alteration of range if $\tan\theta=\sqrt{3}$; that is, if $\theta=60°$. If the direction of firing has any easting in it, $\sin z$ will be positive; and if θ be less than 60°, the range will be increased; but if θ be greater than 60°, the range will be diminished by the rotation of the earth; and *vice versâ*, when the direction of firing has any westing in it. This has been pointed out already, as regards firing due E. or W., by Professor Price; but we see that it holds equally for all azimuths of projection.

(2) In the case of a vacuum and a parabolic trajectory, we could substitute for h, in (8), its value in terms of r, viz. $\frac{1}{4}r\tan\theta$; thus obtaining, for the whole deflection from the rotation of the earth,

$$tr\omega\cos\lambda\,(\tan\lambda+\tfrac{1}{3}\tan\theta\cos z).$$

Hence there would be no deflection if $\tan\theta\cos z$ and $3\tan\lambda$ were equal and of opposite signs. For firing due N., $\cos z$ is -1. Therefore, for firing N. in a vacuum, there would be no deflection if $\tan\theta=3\tan\lambda$; as pointed out already by Professor Price. For N. firing there is, as we know, no alteration of range; therefore, in this case, there would be no shift whatever of the point of fall of the projectile from the rotation of the earth.

CHAPTER IV.

FOUCAULT'S PENDULUM.

This subject, like the last preceding one, though belonging to Chapter II., will be better discussed in a place by itself.

The idea of employing a pendulum, in the manner now to be considered, for the purpose of proving the rotation of the earth, was first proposed and carried out into practice by Foucault in 1851. The pendulum so used has, therefore, come to be called by his name. It consists simply of a heavy bob hanging by a single cord or wire, and free to swing in any direction. If it be set oscillating in a plane, there is nothing to make that plane partake of the earth's component rotation V (see last Chapter) about the vertical line at the locality. As the horizontal surface beneath the pendulum, on which the direction of oscillation is marked, is turning round in its own (instantaneous) plane, counter-watch-wise, with the angular velocity $\omega \sin \lambda$, the plane of oscillation is left behind and will seem to the observer, who is unconscious of his own motion along with the earth, to have a rotation, with that rate, in the opposite direction, or that of the motion of a watch lying face upwards on the table.

We may here note that a reader must be sometimes puzzled by a statement which is often inconsiderately made without any qualification, though nothing wrong be really intended by it. He will find it stated that the Pendulum oscillates always "in the *same plane*" (italics not ours), and that the plane of oscillation "remains always parallel to itself," and that it "always retains its own direction," and that it "is fixed," and that it "has fixity of position," &c. This is so only in the respect just

mentioned, viz. that it does not partake of the earth's component rotation V, nor turn at all about the vertical line as axis. But the plane of oscillation participates, after its own fashion, in the earth's component rotation M about the horizontal meridional line at the place of observation. When that plane is in the meridian, or N. and S., it turns about said line, as axis, with the angular velocity $\omega \cos \lambda$; when it is at right angles to the meridian, or E. and W., it does not turn about that line at all; at that time it really *does*, though for a very short period, "retain its own direction." In general, if z be its azimuth or inclination to the plane of the meridian, its rate of turning about the horizontal N. and S. line is $\omega \cos \lambda \cos z$; the angle z always varying and increasing with the time. It is then inconvenient and, for learners, misleading to speak without reservation of the plane of oscillation as "remaining always parallel to itself," when it has, in reality, the peculiar varying angular movement just described. However, we are free, now, to disregard this movement, as it does not *sensibly* affect the present question.

Foucault communicated an account of his Pendulum to the French Academy on February 3, 1851, which appears in the *Comptes Rendus* for that date. A description of it taken from his own paper will be found also in *Phil. Mag.* 1851, first half, p. 575, and in *Edinb. New Phil. Journ.* vol. li. 1851, p. 101.

Though the main principle of this Pendulum, as propounded by Foucault and stated above, is simple enough and to be called a kinematical one, the complete theory of it, even for a vacuum, presents an exceedingly difficult dynamical problem, one indeed apparently incapable of complete solution. This problem has been investigated by many able mathematicians, from 1851 downwards; perhaps the latest paper on the subject is that by M. De Sparre, "*Sur le Pendule de Foucault*," presented to the French Academy and reported on in the *Comptes Rendus*, April 13, 1891.

The causes of disturbance in the desired performance of this

Pendulum are of several quite different kinds, which, however, cannot be kept altogether separate, on account of their interaction.

The first kind is connected with the setting-up of the instrument. It is obvious that there should be the greatest practicable equality of freedom in all directions at the point of suspension, whether the Pendulum be supported by a cord or wire, yielding by its flexibility or its elasticity; or whether it be by a fine point, say of steel, resting on a very hard smooth surface, say of agate. Deficiency of accuracy in this respect will be of less importance, the greater the length of the Pendulum.

There should be of course very great steadiness and rigidity in the supporting structure; unless this have perfectly equal elasticity in all horizontal directions, a condition not to be easily attained. If the Pendulum be heavy, which for certain reasons it ought to be, and if it be suspended from a beam there will be some small elastic yielding in the transverse, with almost none in the longitudinal direction of the beam. In order to obtain great length in the Pendulum, which is desirable for certain reasons, it has been hung in church-towers, sometimes surmounted by spires. But the elastic swaying of such structures at a considerable height from the ground under the varying pressure of a moderate wind is very appreciable, and in some cases might quite annul the advantage derivable from the great length of the Pendulum. That the instrument should be safe from the direct interference of the movements of the air, it should, as a general rule, be confined in a draught-proof case. The disturbances referred to, so far, may be almost quite avoided by the exercise of very great care and accuracy.

The second kind of disturbance is inherent in the very nature of the Pendulum itself. Suppose it to be set swinging on a non-rotating earth; if the oscillations were exactly in a plane they would, of course, remain so, and the plane would remain stationary. But if they were not in a plane, the bob would describe, in a vacuum, what may be called an ellipse, whose axis-major would continually rotate in the same direction as that in

which the bob was describing the curve. If l be the length of the pendulum and a and b the semi-axis major and minor of the ellipse, both relatively very small, then on a non-rotating earth and in a vacuum, a would accomplish a complete rotation in the time of a whole vibration, or two complete swings of the pendulum (that is $2\pi\sqrt{\frac{l}{g}}$ secs.) multiplied by $\frac{8}{3}\frac{l^2}{ab}$ very nearly. That is to say, the angular movement of the axis-major in one second would be, in circular measure, $\frac{3}{8}\sqrt{g}\frac{ab}{l^{5/2}}$ very nearly. See articles in *Phil. Mag.* 1851, second half, and Williamson and Tarleton's *Dynamics*, p. 214 (see also NOTE A). This result is only approximate, though very closely so, for great enough l, or small enough ab. It would obtain also on the rotating earth, though of course in combination with the effects of the rotation.

In order to keep this disturbance as small as may be, l should be as great and the product ab as small as possible without practical disadvantage. If it were practicable to keep b at zero, that would, of course, be sufficient to keep the above expression for this angular movement so, likewise; but we shall find that this is not practicable, though it can be approached to pretty nearly.

There is another unavoidable source of interference with the desired performance of this Pendulum; which is that, as we have seen, it is affected, though very slightly, by the earth's component rotation M about the horizontal meridional line at the place of observation, and that, therefore, its behaviour is not altogether independent of the azimuth of its mean plane of oscillation. It may be that certain variations in the rate of rotation of that plane, as described by some experimenters, have been, to some extent, due to this circumstance. Let us note the following for the sake of illustration; though it is sensibly quite unimportant. The rate of that angular movement (in a vacuum) of the line of apses mentioned above is, as we have seen, proportional to $\sqrt{g}$, *cæteris paribus*; g being the whole downward acceleration, in-

cluding that of the centrifugal force from the rotation of the instrument connected with M. But we have seen that when the Pendulum is swinging N. and S. the downward centrifugal force is a maximum, and when the Pendulum is swinging E. and W. that force is zero. Therefore, if this effect could exist by itself, the line of apses would move very slightly faster when near N. and S. than when near E. and W. As another illustration, we may observe that the behaviour of this Pendulum is not entirely independent of the azimuth of oscillation with which it is started. We shall meet with still another illustration further on.

The gyrostat, when used to prove the rotation of the earth, is quite free from such complications as those now referred to.

The third kind of interference with the desired performance of this Pendulum is that arising from the resistance of the air. For very small velocities, this resistance would be directly proportional to the velocity, very nearly; if there were not anything to prevent this. But there *is* something to prevent this; for as the amplitude of swing must be kept small and the axis-minor of the ellipse exceedingly small, the Pendulum is always moving in air which has been already disturbed by itself. If it were moving in a wide enough ellipse to avoid this, the resistance of the air, if acting by itself, would cause a retrograde movement of the apses of the ellipse; but in the case of a quite small axis-minor this would be lessened by the movement of the air following in the wake of the bob. There is then reason for believing that, in this case, *this* effect of the resisting air is unimportant. See NOTE B. But there is another which, though it is indirect, is of much more consequence. While the axis-minor is small, but appreciable, the stream of air following in the wake of the bob in one swing will not act centrically and directly against the bob in its return; but it is evidently always tending to turn it away from the axis-major; this is strongest while the bob is descending towards the axis-minor, and the effect is to increase the axis-minor. This tendency must grow with the growth of its own

result, until the ellipse becomes wide enough for the cause to cease. This is, no doubt, one reason why the axis-minor (unless it be exceedingly small) grows larger, at first absolutely, and then relatively, during the continuance of an experiment with this Pendulum.

It would therefore be impossible to calculate the effect of the resistance of the air on the behaviour of the instrument, as the precise conditions of it are unknown and altering continuously with the lapse of time.

To diminish as much as possible the relative importance of the air, the bob must be, of course, as large as convenient and of high density. It should also be very homogeneous and carefully turned in a lathe and suspended accurately in its axis of figure.

We have seen that, besides the precautions necessary in the making and mounting of the Pendulum, there is the very important one of starting it properly, so as to have as small an axis-minor of its path as possible. For this purpose the plan has been generally followed of starting the Pendulum by drawing it to one side by a thread attached to a stationary object, and when the Pendulum has come to rest of severing the thread by burning it. But, on account of the rotation of the earth, the centre of the bob will in this case pass to the right of the point of rest in northern latitudes. The plan has therefore been adopted of projecting it from the point of rest with the view of making it swing to and fro through that point. But supposing that it did this at first, it would describe, relatively to the table beneath it and to the accompanying air, a series of loops all described in the same direction (and therefore not "figures-of-8," as sometimes called), and the tangential resistance of the air near the outer ends of the loops, although excessively small, would, by continued action in the same direction and by accumulation of effects, cause the axis of the bob to pass to the right of the central point of rest. If the linear amplitude of oscillation were too large, this might well have a quite sensible effect.

It is therefore all important, in experiments with this instru-

ment, to use as small an amplitude of oscillation as practicable; in order to diminish, as much as possible, three quite different causes of disturbance noted above. This was not sufficiently attended to at first.

It should be remembered that any roughish experiments with Foucault's Pendulum are necessarily quite delusive. In consequence of insufficient guarding against the causes of disturbance, it has happened, even with some experiments considered worthy of being described in a scientific journal, that the line of apses has actually gone the wrong way! This has, not unnaturally, given occasion to certain persons, including the famous "*Parallax*," to ridicule the principle of this Pendulum altogether.

The later experiments of Mr. Thomas G. Bunt, of Bristol, described by himself in different papers in the *Phil. Mag.* for 1851 and 1852, were carried out with unusual care to minimise the causes of disturbance, and they were, for that reason, specially successful. He started with a linear amplitude of swing of only one inch on each side of the point of rest. He mentions that (the axis-minor of the ellipse described being always kept very small) all his Pendulums had two nodal lines nearly at right angles to each other, at which the direction of revolution of the bob in the ellipse changed to the opposite. This affords another illustration of the fact that this Pendulum is not altogether indifferent to the azimuth of its mean plane of oscillation.

An interesting table of results obtained by various experimenters with Foucault's Pendulum will be found in pp. 44, 45 of Rev. Dr. Haughton's *Manual of Astronomy*.

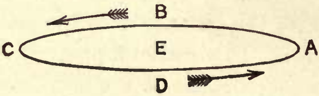
NOTE A, from p. 63.—That the axis-major of the ellipse must rotate (in a vacuum) in the direction in which the Pendulum describes the curve can be seen quite easily without analysis. The force directed to the point of rest, under which the Pendulum is oscillating, is accurately $g \sin \theta$; θ being the angular distance from the point of rest. Therefore when θ is very small, the

Pendulum is moving under a central force which is very nearly indeed directly proportional to the linear distance; it therefore describes very nearly a fixed "central ellipse." But, from the exigencies of the experiment, θ cannot be allowed to be exceedingly small; and therefore the force, which is proportional to $\sin \theta$, varies, as is evident, more slowly than the distance, whether linear or angular, from the point of rest; and the deficiency in the central force, owing to this, which is at first excessively small, increases with the distance from the point of rest and at a much higher ratio. This causes a progressive motion of each end of the axis-major; because in the neighbourhood of the apse, where the deficiency is greatest, the central force takes longer to stop the rising of the bob from the centre of force and to pull it round the apse than it would do if it were accurately proportional to the distance; the bob will not attain its apse, and begin to turn back again, until it has passed the position of the last preceding corresponding apse. For a corresponding contrary reason, the said deficiency in the central force, as occurring near the ends of the axis-minor, would tend to produce a retrograde motion of each of those points. But the former tendency is greater than the latter; since the said deficiency is greater at the ends of the axis-major than at those of the axis-minor, in a much higher proportion than the distances from the centre of the ellipse. The importance of this consideration is enormously enhanced by the fact that the axis-minor must be always kept very small. The whole result is consequently a progressive rotation of the ellipse.

Note B, from p. 64.—That the resistance of the still air, if it could act separately, would cause an angular movement of the axis-major in the direction contrary to that in which the bob describes the ellipse, can be seen in a similar manner. See Fig. 12, in which the axis-minor is, for clearness, made greatly too large in proportion. Whilst the bob is going from D to A, the resistance of the air, which is tangential to the curve, tends

to make A regress; because it causes the bob of the pendulum to cease rising from E, and begin to turn downwards, sooner than it would do without that resistance; that is before it has reached the last preceding position of A. But whilst the bob is going from A to B, the tangential resistance tends, in a corresponding manner, to make B progress. The former effect, however, exceeds

Fig. 12.



the latter; because whilst the bob is rising from D to A its velocity and the consequent resistance of the air are at their maximum *at first*; but whilst the bob is going from A to B the velocity and the resistance only reach their maximum *at last*. The whole result will be that the "ellipse" would rotate retrogressively if the resistance of the still air were the only disturber of the elliptic motion. This is corroborated by the experiments of Mr. Alexander Gerard. However, if the ellipse be narrow enough, the last-mentioned effect will evidently be increased by the resistance of the wake-stream; so that the whole effect may be quite small.

CHAPTER V.

ON THE POSITION OF THE DYNAMICAL HIGH TIDE RELATIVELY TO THE CELESTIAL TIDE-PRODUCING BODY.

As is often done for simplicity, we shall consider only the tides that would be produced in a canal of uniform depth and of uniform width running right round the earth's equator and returning into itself; and we shall suppose the tide-producing heavenly body to be always in the plane of the equator. We shall, moreover, confine our attention, at first, to the tides caused by the moon.

We need not do more than remind the reader that the lunar tidal forces are directed as the outer broken-line arrows in Figs. 15 and 16, the moon being away to the right, and that they consist only of the differential attraction of the moon on the water of the ocean, or the difference, both as to magnitude and direction, between her attraction at the centre of the earth and at the various parts of the superficial ocean. The tangential tidal force at a point on the earth's surface having the angular distance θ from the moon is $\frac{3}{2}\frac{rM}{R^3}\sin 2\theta\gamma$; and the radial force at that point is $\frac{3}{2}\frac{rM}{R^3}(\cos 2\theta+\frac{1}{3})\gamma$; r being the earth's radius, M the moon's mass, R the moon's distance from the earth, and γ the unit of gravitation. These forces are, then, inversely proportional to R^3. The differential tidal force is at its maximum directly under the moon, where it is all radial, and where it is only about 1/29th of the moon's whole attraction at the distance

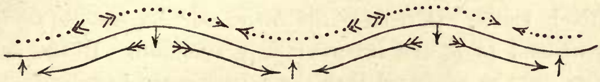
of the earth, or about 1/8,400,000th of g, or the earth's attraction at its surface. If the earth always kept the same side turned towards the moon, the lunar tidal forces would, of course, produce one tidal protuberance in the water on the side of the earth next the moon, and another on the opposite side. The protuberances would be stationary on the earth, and the discussion of their magnitude &c. would be one of hydrostatics only; they are therefore called statical tides, or equilibrium tides.

But as the earth rotates under the moon, the actual case in our equatorial canal would be very different. The two tidal protuberances and intervening depressions, in order to keep up with the moon, would have to sweep right round the canal in the mean period of 24 hours 50·5 minutes, at the rate of 1003·5 miles per hour. This they would do, not after the manner of a tremendous torrent moving bodily along with that enormous velocity, but in the style of a smooth ground-swell in the sea, whose gentle wave-forms may be travelling onwards with a considerable speed, although the individual particles of the water are only moving backwards and forwards, for short distances, with quite small velocities. This is the manner in which the actual tides in our oceans really do travel. We are therefore concerned with a dynamical question, and have to do, not with "statical," but with "dynamical," tides. The present subject is one on which it is very easy to go wrong; it contains several instances of what any person insufficiently acquainted with it would naturally regard, at first sight, as apparent paradox.

Let us begin by noting briefly the way in which the water moves in a travelling wave, or water-undulation. Anyone can observe this for himself when watching sufficient wind-waves on the sea; although such surface undulations differ importantly in certain respects from tidal ones, whose disturbances extend to the bottom of the ocean. See Fig. 13, which represents two waves moving towards the right. The upper dotted arrows

show the directions of the movement of the various parts of the water. The lower arrows the directions of the gravitation forces due to the disturbance of level. On the crest of the wave the water is moving horizontally forwards with the greatest velocity; at the bottom of the trough the water is moving horizontally backwards with the greatest velocity. At the points of mean

Fig. 13.



level, halfway up the slopes of the wave-ridge, the water is moving neither forwards nor backwards, but on the front slope, vertically upwards; while proceeding to form the upper part of the ridge by addition in front; and on the hinder slope, vertically downwards; while withdrawing from the hinder part of the wave-ridge. In a wind-wave each particle of water moves in a fore-and-aft vertical circle; in a tide-wave in a very elongated ellipse with minor axis vertical; *this* axis diminishing as we descend, until it vanishes at the bottom. The progress of the wave form is produced by continual addition of water in front, and subtraction of water behind. It is very easily seen that the velocity of the wave-form, though so entirely different from that of the particles of the water, will be proportional, *cæteris paribus*, to the latter; and also that for a given velocity of the wave-form, its magnitude will increase or diminish in the same proportion as the velocity of the particles of water.

Such a wave, having been started by some cause, would, on the cessation of that cause, continue to move onwards of itself, at its own proper rate, in consequence of the forces occasioned by the disturbance of level. There would be the unbalanced weight of the part of the wave projecting above mean level, and the unbalanced deficiency of weight in the part below mean level resulting in an upward pressure in that part, Fig. 13. It is evident that the said pressure and deficiency of pressure is

proportional to the volume of water above, and deficiency thereof below mean level; that is to say (the oscillations being relatively small), proportional to the greatest heights and depressions of the water. The forces are then always proportional to the distance from the position of rest; as in the case of a common pendulum oscillating with a relatively small linear amplitude; and the oscillations are therefore isochronous, or performed in equal times, whatever be their magnitude; if this be always relatively small. Of course the forces will be, *cæteris paribus,* proportional to g, the intensity of gravitation. If the deformation were so produced that the prominences and depressions, when left to themselves, would have no horizontal motion, the wave-forms (though not all the water) would simply oscillate up and down, constituting stationary waves. But if started to move in either direction, they would continue to move, of themselves, in that direction, at their own rate, until their motion was destroyed by friction.

The above-mentioned unbalanced weight and deficiency of weight in different parts of the wave, acts in a two-fold manner. While the weight of the prominences tends to depress them, and, by hydrostatic pressure, to force outwards the water of the intervening parts below the mean level, the tangential component of gravitation on the more superficial parts of the water on the wave-slopes is part of the whole motive force. The radial (or vertical) forces, whether downward or upward, and the tangential forces resulting from gravity conspire with each other in causing the movement of the water of a free wave; and therefore the whole effect is the same in *general* character (which is all that now concerns us) as though the gravitation forces were entirely tangential.

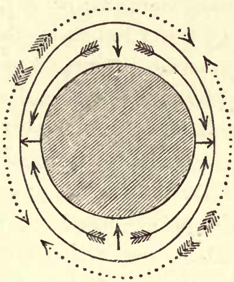
This is true of the lunar tidal forces also; the radial and the tangential conspire with each other in their constant effort to lower the water at 90° away from the moon, and to raise it under, and on the off side from, the moon; their whole general effect is the same as if they were entirely tangential. This con-

sideration is strengthened by the fact that the tidal effect of the lunar radial (or vertical) forces is quite insignificant as compared with that of the tangential ones.

Therefore, considering what our present object is, we may, if convenient, treat both the gravitation forces and the lunar tidal forces as though they were wholly tangential; and it will be very convenient to do so presently.

The two tidal waves with which we have to do constitute what we shall call an ellipse, it being nearly such; as represented by the ellipse in Fig. 14, which is Fig. 13 adapted to our

Fig. 14.



present purpose. They are supposed, in the diagram, to be moving, or revolving, relatively to the body of the earth (represented by the shaded circle), in the direction of the hands of a watch. The dotted arrows outside the ellipse represent the horizontal movements of the water itself; in accordance with what we have described above as the movements of the water in a wave. The arrows within the ellipse represent the positions and directions of the tangential and radial gravitation forces. The tangential forces are acting throughout one half of their reach, or extent, concurrently with, and through the other half against, the motions of the water which would be produced by them in a free wave; as with all ordinary oscillations, for instance those of a common pendulum.

Now it so happens that the general scheme of the lunar diffe-

rential forces all round the earth, as regards their positions and directions relatively to each other, is similar to that of the above-mentioned gravitation forces ; so similar that if the moon be supposed to be opposite a *side* of the tidal ellipse, the members of the two sets of forces will, with a trifling exception mentioned below, respectively agree in direction and act together. The gravitation forces of the tidally-deformed water, shown by the inner arrows in Fig. 14, produce, as we have seen, the motions of the water shown by the outer dotted arrows in that diagram. It is evident, then, that the lunar tangential forces, whose scheme is similar, if they could act by themselves, without calling into being the gravitation forces, would produce, under the condition of the rotation of the earth beneath the moon, a similar system of movements of the water, whose directions would be represented by the said outer arrows in that diagram, and whose relations would be very nearly those of the different parts of a great ocean wave whose length was equal to a semi-circumference of the earth.

Thus the actual tidal waves move under the influence of a scheme of lunar forces, acting along with a generally similar scheme of gravitation forces, which they themselves have occasioned. (In the present chapter we are quite unconcerned with the trifling differences of detail which exist between the lunar and the gravitation forces. The only one worthy of mention is that whilst the very slightly operative lunar radial (or vertical) force vanishes at 54° 44′ from the moon, the gravitational radial disturbing force vanishes at the mean level of the water, which, without friction, would be 45° from the moon, very nearly, and, with friction, differently situated, as will be seen from pages 81 and 82 below.)

One considerable difficulty in understanding the production of the dynamical tides arises from the coexistence and cooperation or antagonism, as it may be, of these two systems of forces.

Let us note now that if v be the velocity with which a free, frictionless undulation of the water, reaching to the bottom

and of very great length relatively to the depth of the water, would travel, of its own accord, $v = \sqrt{dg}$; d being the depth of the water and g gravity. In order that such undulation should so travel with the mean velocity necessary for its keeping up with the moon, at the equator, viz.: 1003·5 miles per hour, the depth of the water $(= v^2/g)$ should be 12·76 miles. As there are two complete tides in every lunar day of about 24 hours 50 minutes the mean period of a single tide is 12 hours 25 minutes, very nearly. If the depth of the water were less than that just mentioned, a free tidal wave could not keep up, of itself, with the moon; and its period of oscillation would be greater than 12 hours 25 minutes; if it keeps up with the moon, as it would have to do, it must be as a "forced wave," forced by lunar tidal action. But if the depth were greater than the depth just mentioned, the period of a free tidal oscillation would be less than that of the forced lunar tidal wave; if it keeps pace with the moon, as it would have to do, it must be again as a forced wave, but one whose velocity is restrained by the lunar tidal action. The depth now in question we shall call the *critical depth.* (That is for the *equatorial* canal. If the canal ran along the parallel of latitude λ, the velocity necessary for keeping up with the moon would be $1003{\cdot}5 \cos\lambda$ miles per hour; and the critical depth would be $12{\cdot}76 \cos^2\lambda$ miles.)

What then will be the position of the lunar dynamical high tide, relatively to the moon?

This is really a manifold question, which requires four different answers, according as the water is supposed to be with, or without, viscosity, or friction; and as the depth of the water (always uniform) is supposed to be less, or greater, than the critical magnitude just mentioned. We shall consider afterwards the case when it is of that magnitude.

First, then, let us suppose that there is no friction, or viscosity, in the undulating water.

A 1. Let the depth of the frictionless water be less than 12·76

miles, the critical depth, so that a free tidal wave would oscillate more slowly, that is, with a greater period, than the forced tidal wave. In this case low water of the dynamical tide will be under the moon; that is, high water (which for the statical tide would be under the moon) will be 90° behind, or east of, the moon.

A 2. But let the depth of the frictionless water be greater than 12·76 miles, the critical depth, so that the free tidal wave would oscillate more rapidly, that is with a shorter period, than the forced lunar tidal wave, then high water of the dynamical tide will be under the moon; that is, it will occupy the same position, relatively to the moon, as high water of the statical tide.

Both these cases are comprehended in Airy's general mathematical expression for the height of the water of the frictionless dynamical tide in an equatorial canal, at a given angular distance from the moon. (See Note A.)

Airy proposes the following interesting illustration of this:—If there were two equatorial canals, such as the above, side by side, to all appearance similar, one, however, being less and the other more, deep than the critical depth, then, with frictionless dynamical tides, high water in one canal and low water in the other would run abreast. (See Note B.)

We can, for ourselves, put the explanation of this into the following simple form, which will be found to be quite sufficient; although it does not go into any details of the movements of the water.

[N.B. We shall sometimes, for brevity, speak of water which is of less than the critical depth as "shallow" water, and of that which is of greater, as "deep" water.]

Let us begin with considering a simple example which illustrates the general principles involved.

A pendulum is hanging at rest; it would have its own proper period of vibration under the influence of gravity. Now suppose

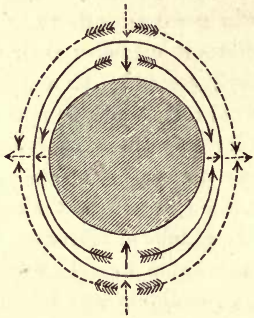
it to be acted upon by a system of small reciprocating *impulses* which have a different period, and whose magnitude is independent of the amplitude of the vibrations and constant, the *forces* of the impulses varying between zero and *maximum* according to their own law, and symmetrically on each side of the point of rest of the pendulum. The amplitude of the vibrations will increase by accumulation, and the tangential gravitation forces called into being by the excursions of the pendulum from the position of rest, and proportional thereto, will also increase. They will soon become great enough to be able, by the baffling effect due to their efforts to establish their own vibration period, to prevent any further increase in the amplitude of the vibrations under the small external reciprocating impulses, which, as we have said, remain of constant magnitude. It is evident that the smaller the difference between the period of the pendulum, if free, and that of the impulses, the less will be the said baffling effect, and the greater the final amplitude of vibration. When the amplitude has arrived at the maximum (equal on both sides of the point of rest) for the given pendulum and for the given reciprocating impulses, the final, settled state of things is reached; the period of vibration being that of the impulses. The two systems of forces will be both symmetrical on each side of the position of rest of the pendulum, and therefore so with each other.

So must it be with the scheme of gravitation forces created by the tidal deformation of the surface of the water of the equatorial canal and the scheme of the lunar tidal forces. They must get into such a final relative position that their respective axes of symmetry will coincide; and this, of course, involves the coincidence of the axes of the tidal ellipse and those of the scheme of disturbing lunar forces; leaving the question still to be settled in which of the two possible ways the coincidence will occur in the particular case; whether as in Fig. 15, or in Fig. 16, the moon being away to the right. Either the longest or the shortest axis of the tidal ellipse must point directly to the moon. As before, the shaded circle is the body of the earth, and the

ellipse the surface of the water, the ellipticity being enormously exaggerated. The apparent motion of the moon, or that relative to the surface, is always watch-wise.

Take now the case of a "shallow"-water tide in the equatorial canal, which would spontaneously oscillate and travel more slowly than the moon would have it to do. As we have said, the motive force, causing the spontaneous free oscillations of the

Fig. 15.

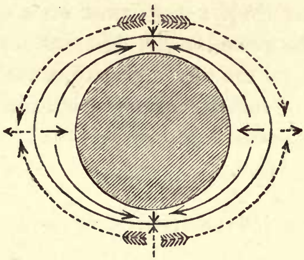


water, is the weight of the high-tide prominences and the deficiency of weight of the intervening low-tide depressions. Now it is evident that the tidal wave has to travel at the moon's rate; however this be brought about. In order that the "shallow"-water tide may oscillate and travel quickly enough for this, it must become so situated relatively to the moon that its own just-mentioned motive forces shall have the moon's tidal forces helping them; and it is evident that this will be so when the middle of a side of the tidal ellipse is, at least, nearly opposite to the moon. That is to say, low water must be, at least, nearly under the moon; and from what we have seen above, if it be nearly so, it must be directly so, as in Fig. 15; and this of course applies equally to all depths of water less than the critical depth.

Take now the case of a "deep"-water tide, which would, if

free, oscillate and travel more quickly than the moon would have it to do. It must travel at the moon's rate; however this be brought about. In order that it may move slowly enough to keep pace with the moon, it must get into such a position relatively to the moon that its own motive forces shall have the moon's tidal forces opposing and restraining them; and of course this will be so when the end of the tidal ellipse is, at least, nearly opposite to the moon. In other words, high water must be, at least, nearly under the moon, and therefore directly so, as in Fig. 16, and this manifestly applies equally to all depths of water greater than the critical depth. (See NOTE C.)

Fig. 16.



Thus the summit of a "deep"-water dynamical tide would occupy the same position, relatively to the moon, as that of a statical tide. But the magnitudes of the tides would be generally different. If the water were not too much deeper than the critical depth, the dynamical tide would be the greater; but if the water were deep enough, the statical tide would be greater; and of course for a certain intermediate depth they would be equal.

We may here note the following:—If two canals of uniform width and depth ran side by side, along two parallels of latitude not too close together, each returning into itself, and if they were both of the critical depth corresponding to the mean latitude λ, which depth would be, as we have seen, $12{\cdot}76 \cos^2\lambda$

miles, then high water of one canal and low water of the other would run abreast; since the more northerly canal would be deeper, and the more southerly shallower, than its own critical depth.

Perhaps it might be thought that if the water, as under the present supposition, were absolutely frictionless, there would be nothing to prevent the protuberances of a statical tide sweeping round the earth bodily, in the manner of a solid mass, at the angular rate of the moon, with their summits always under, and on the off side from, the moon. It is quite true that such a tide of the proper magnitude formed and set going *by some other agency* to so travel with perfect accuracy, would be kept up by the moon, and would preserve its position relatively to the moon. But the moon itself could not so start such a tide; because the varied action of the moon, on the different parts of the hitherto undisturbed water on the rotating earth, would produce therein, at once, a system of varied movements agreeing very nearly with that of a free wave-motion (see p. 74); thus creating immediately a dynamical tide.

Now let us recognize the friction, or viscosity, of the undulating water.

B 1. When the depth of the water is less than the critical depth (so that a free, frictionless, tidal wave would move more slowly than the moon would have it do), the effect of the addition of friction, paradoxical as it might seem at first sight, is to make high water to be *before* the place, relatively to the moon, that it would occupy without friction; that is to say, high tide would be somewhat less than 90° behind, or east of, the moon; and it would occur sooner in time than it would for frictionless water.

B 2. On the contrary, if the depth of the water were greater than the critical depth (so that a free, frictionless, tidal wave would move more quickly than the moon would have it to do), the effect of added friction would be to make the point of high

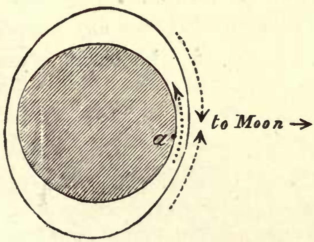
water to be *behind* the place, relatively to the moon, that it would occupy without friction; that is, it would be a little distance behind the moon, instead of being directly under her, as it would be without friction, and it would come later in time than for frictionless water.

The analytical proof of these two statements will be found in pp. 331* and 332* of Airy's Art. referred to in NOTE A. Though he omits, in that place, to state B 2 for us, he enables us to do this for ourselves. We may observe that all four statements A1, A2, B1, and B2 will be found in Prof. George H. Darwin's Art. "Tides" in the last edition of the *Encyclopædia Britannica.*

The geometrical proof of B1 and B2 is quite simple. For it we must now turn to the consideration of the movements of the different parts of the water of the tidal wave; for it is on this that friction depends.

First we take case B1, availing ourselves of the mode of proof given by Rev. T. K. Abbott, Fellow of Trinity College, Dublin.

Fig. 17.

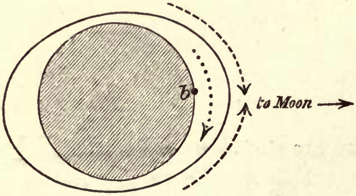


Suppose that we are standing on the ground beside the canal at a, Fig. 17; the body of the earth rotating under the moon counter-watch-wise; as we are carried on towards the point under the moon, the velocity of the tidal current indicated by the dotted arrow is increasing under the continued action of the lunar tangential force indicated by the broken-line arrow; and therefore the frictional resistance due to the current is increasing

in the opposite direction; in addition to this, the tangential lunar disturbing force, which has been, and is, giving the water its increasing velocity, is itself diminishing. The friction-resistance will therefore become equal to the oppositely directed lunar tangential force, somewhat before this force becomes zero; that is, at a point short of that under the moon. At that point, then, the *whole* tangential force passes through zero, and changes its direction, and begins to pull the water backwards against that behind it; thus causing it to cease falling sooner than it would do without friction, and at a point ahead of that under the moon. And, for a similar reason, high water will occur at a point short of, *i.e.* ahead of, 90° behind the moon. Thus, as in Fig. 17, the axis-minor of the tidal ellipse will not point to the moon. This acceleration of the phases of the tide is evidently at the expense of some of the magnitude of the tide. The tide ceases to fall before it has reached what would be its lowest point without friction.

In case B2, as we can easily see for ourselves, the contrary takes place; because the directions of the tidal currents, both under the moon and 90° away, are the opposite of what they are in case A1. Suppose that we are standing on the ground beside the canal at *b*, Fig. 18, which has not yet reached the point

Fig. 18.



under the moon. As we are carried by the rotation of the earth near to that point, the lunar tangential force is slowing the current; and the friction-resistance, now near its maximum, conspires with it in so doing. When we have reached the point

under the moon the lunar tangential force has vanished; but the current, of course, continues, and the friction continues slowing it, and though the lunar tangential force begins, at that point, to act in the opposite direction, and against friction, it will not become equal to it (and then greater than it) until we have been carried by the rotation of the earth more or less behind the moon, as in Fig. 18. Therefore the greatest slowing is at a point behind that under the moon; and there high water will occur. And, for a similar reason, low water will be similarly retarded; and, as in Fig. 18, the axis-major of the tidal ellipse will not point to the moon.

Or thus:—Friction, in the case of a "shallow"-water tide, prevents the full formation of the *hinder* parts of each tidal prominence, and of each tidal depression; thus making the highest and lowest points of the water to be ahead of the places they would occupy without friction; but this, as is evident, is at the sacrifice of some of their height and depression, respectively.

On the other hand, in a "deep"-water tide, the friction prevents the full formation of the *front* part of the tidal prominence and of the depression, with, of course, a contrary result; and again at the sacrifice of some of the height and depression of the tidal wave.

It is evident that the forward, or backward, shift of the position of low, and of high, water will be greater, *cæteris paribus*, for a greater coefficient of friction; though only up to a certain limit to be mentioned later on, p. 86. It will also be greater, *cæteris paribus*, for a nearer approach to equality between the periods of free, and of forced, oscillation, or for a nearer approach of the depth of the water to the critical depth.

But now let us ask, if the frictionless water were just of the critical depth, what would be the position of high water relatively to the moon? The principle appealed to above will help us to answer this question as well as it can be answered. The moon would not then be forcing the water to oscillate, and the tidal

wave to travel, at a rate different from its spontaneous rate under gravitation; and therefore the lunar tidal forces would have to be, on the whole, neither helping nor opposing gravity, as regards affecting the oscillation-rate of the water. It might, then, seem that if there were a tidal ellipse, the four points of half tide, where the water is at the mean level, should be at the points under and opposite to the moon and 90° before her and behind her; that is to say, the high tide, which statically would be under the moon, should be at the point of 45° behind, or east of, the moon. But the case would be a peculiar one. If the depth of the water were ever so little less than the critical depth, high water would be 45° behind this said point; and if the depth were ever so little greater than the critical depth high water would be 45° before that point. Thus, even though it were mathematically possible, so to speak, that the point of high tide should remain at 45° behind the moon, it would not be practically possible; because the condition would be one of instability. But, moreover, even if a tidal wave in water of the critical depth could be, by some means, formed with its crest 45° behind the moon and started so as to keep up with her, the lunar tangential force (vastly more important than the radial) would be, as is evident, continually helping gravity in the front part of the wave and opposing it equally in the hinder part. The result would be such confusion as would destroy the wave before long. We have just seen that it would be impossible for the crest of the wave to remain at any other point *within* the first quadrant behind the moon; it must be either 90° away from the moon or under the moon, with an equal right to both positions. Since the period of the alternating lunar disturbing forces would be the same as that of the free oscillation of the water, it is evident that those oscillations, if they existed, would become infinite, but for certain conditions of the case which would prevent this.

The above conclusion, drawn from simple geometrical considerations, is in accordance with that which Airy derives from his equation given in Note A, which see. He observes that if i and

n become equal, that is if d and c, in our simplified form of the equation, be equal, that is if the water be of the critical depth, the tide would be theoretically infinite, and the equation fails. His interpretation of this failure is that the motion of the water would not be oscillatory in the manner of a wave; but that it would be that of a torrent of unequal depth passing round the earth so as to follow the apparent motion of the moon.

The introduction of friction would, however, of itself, keep the height of the tide finite, even though the water were of the critical depth. Since friction always increases with the velocity of the oscillating water, which velocity would obviously increase with the magnitude of the tide always going at the moon's rate, friction would increase with the latter (see same NOTE); and therefore the magnitude of the tide with friction could not increase beyond the reasonable limit at which the general result of the increasing friction in keeping down the magnitude of the tide became equal to that of the lunar forces in accumulating, or piling it up.

It is generally considered that in the actual, relatively very small, tides of the ocean (away from shores), because of the smallness of the velocity of the particles of water, the friction is nearly proportional to the simple velocity of those particles. But in the present supposed case, in which the tides would be very much larger, and in which the velocity of the water would be correspondingly great, the friction would be probably nearly proportional to the square of the velocity; and as the forces of free oscillation would be very nearly proportional to the distance of the summit of the tide from the position of rest, the oscillations, in this case, would be still very nearly isochronous, for different amplitudes, and moreover their period very slightly altered (in accordance with a well-known dynamical principle illustrated by a pendulum with small amplitude of oscillation, whose period is sensibly unchanged by the resistance of the air, if this varied as the square of the velocity). Thus, while the friction would keep down the magnitude of the tide within reasonable limits, it

would alter very little the period of the free undulation of the water; and consequently the critical depth of the water with *such* friction would be nearly the same as that for water without friction.

Suppose the depth of the "shallow" water to increase gradually, the magnitude of the tide will increase faster than the depth; and therefore the velocity of the water will increase, and so will the friction (see Note A); and therefore the shift of the point of high water will increase. But as the friction prevents the height of the tide, and therefore the velocity of the particles of water, from becoming indefinitely great, it indirectly prevents its own self from becoming so; and therefore the summit of the tide could never get within a certain distance of the point of 45°, even though the water attained to the critical depth; said distance depending, as we know, on the magnitude of the coefficient of friction. Similarly, if a "deep"-water canal shallowed gradually to the critical depth, the summit of the tide could never get within the same distance of the point of 45°; and the limits between which it would be impossible for the high tide to remain would be much closer than before.

We have seen that, in the "shallow"-water tide, acceleration of the various phases of the tide is, *cæteris paribus*, greater as the coefficient of friction is greater. But it will be easily seen, on consideration, that no amount of friction in a "shallow"-water tide would be able to make the angular displacement of high water produced thereby as much as 45°. As long as the water is of less than the critical depth, the moon must be forcing the tidal wave to travel faster than it would do of itself; she must be, on the whole, working with the gravitation forces to accelerate the oscillations of the water; and, as is evident, she will not be doing this unless the end of the tidal ellipse is more than 45° behind her. To this we may add that the confusion mentioned in p. 84 would become important if the crest of the tide were sufficiently near the point of 45°, and would help in preventing its reaching that point.

A corresponding statement, *mutatis mutandis*, is, of course, to be made respecting a "deep"-water tide. No amount of friction would be able to make its high water fall back to 45° behind the moon. (See NOTE D.)

It is important, for certain reasons which need not now be mentioned, to note particularly the conclusion from the above—what indeed has been already stated by Prof. G. H. Darwin, *ubi supra*, page 375—, viz., that whatever be the depth of the water, and whether there be, or be not, friction, the crest of that dynamical tide whose position, if it were a statical tide, would be under the moon, can never be outside the first quadrant behind the moon; and that, if there be friction, it must always be within that quadrant.

It is, perhaps, more important, for reasons which need not be mentioned, to note particularly that, as we have seen, whatever the depth of the water, and whether there be or be not friction, the crest of the dynamical tide can never be at the point of 45° behind the moon.

All the above, of course, applies equally to the solar dynamical tide in an equatorial canal; except that for this tide, whose period is 12 hours, and whose rate of progress would be about 1037·4 miles per hour, the critical depth ($=v^2/g$) would be greater, viz., about 13·67 miles, and also that as, *cæteris paribus*, the friction of the smaller solar tide would be evidently less than that of the lunar in a higher ratio than that of the respective tidal forces (as well as for another less important reason), the shift of the points of high and of low water, on account of friction, would be less than that for the lunar tide.

Airy points out the interesting conclusion that if the depth of the equatorial canal were between the lunar and the solar critical depths, that is between 12·76 and 13·67 miles, and there were no friction, since high water of the lunar tide would be under the moon, and low water of the solar tide under the sun, spring tides would concur with the quadratures, and neap tides with the syzygies, of moon and sun; the reverse of what now obtains.

To this we may add, for ourselves, the following respecting the position of high water of spring and of neap tide, when the actual depth of the frictionless water is between the two critical depths. High water of spring tides would be always under the moon, and therefore 90° away from the sun. But the position of high water of neap tide would depend on circumstances. As long as the lunar tide was greater than the solar, high water of neap tide would be under the moon and sun. But if the actual depth were sufficiently nearer to the solar than to the lunar critical depth to make the solar tide greater than the lunar, then high water of neap tide would be 90° behind sun and moon.

NOTE A, from p. 76.—Airy's equation for K, the distance from the mean level of the surface of the frictionless dynamical tide in a uniformly deep and wide equatorial canal returning into itself, may be found in his Art. on *Tides and Waves* in *Encycl. Metrop.* vol. v. p. 322 *. It is, after setting aside a certain term which is relatively quite insignificant,

$$K = -\frac{H}{gm} \cdot \frac{n^2}{i^2 - n^2} \cos(it - mx), \quad . \quad . \quad . \quad (A)$$

H being the moon's tangential tidal force at its maximum (or $g/11{,}660{,}000$), which it attains at 45° away from the moon; m being 2π divided by the length of the wave, which length, at the equator, is the semicircumference of the earth; making $m = \frac{2}{R}$, at the equator (R being the earth's equatorial radius); i is to n inversely as the period of the forced wave (or $12^{h}\,25^{m}{\cdot}5$) to that of the free wave, for the actual depth of the water; that is directly as $\sqrt{c}$ to $\sqrt{d}$ (c being the critical and d the actual depth); and $it - mx$ is 2θ; θ being the angular distance of the point in question from the moon. Therefore the above equation can be written thus, in a form more convenient for our present purpose, giving the value of K in feet:

$$K = -\overset{\text{ft.}}{0{\cdot}90}\frac{d}{c-d}\cos 2\theta. \quad . \quad . \quad . \quad . \quad . \quad (B)$$

This expression, like (A), from which it follows, is only approximate; and therefore for certain purposes it would not be right to give it too great a range of application. But the following can be derived from it.

Selecting the point under the moon where $\cos 2\theta$ is 1 and a maximum, which makes K a maximum, whether positive or negative, if the depth of the water be less than the critical depth, the denominator is positive, and K is negative; *i.e.* low water is under the moon; but if the depth be greater than the critical depth, K will be positive, and high water will be under the moon. We see also that if the depth be small relatively to the critical depth (but only on that condition), the height of the tide varies nearly as the depth; that is, in a slightly higher ratio than the depth. We see also that, if the water be of the critical depth, K is theoretically infinite, and the expression fails.

We may note also the following:—It is easily seen that if the depth of the water were to increase gradually, and if the magnitude of the tidal wave increased in the same proportion, the velocity of the particles of water would be constant, and the friction constant. But the above equations show that if the depth of the water increased, the magnitude of the tide would increase in a higher ratio, and therefore friction would increase.

We see also from equation (B) that if the actual and the critical depths be not very different, the height of the tide will vary nearly as the inverse of the difference. Hence, if the actual depth were between the lunar and the (greater) solar critical depths, or if it were greater than both those depths, the solar tide, which is now about $\frac{2}{5}$ of the lunar, would be the greater of the two, if the actual depth were sufficiently nearer to the solar than to the lunar critical depth. This, however, may be called self-evident after some of the considerations adduced in the text.

NOTE B, from p. 76.—In Airy's well-known geometrical proof of the position, relatively to the moon, of the frictionless

dynamical high tide (*M. Nots.*, R. A. S., vol. xxvi. p. 229), the writer had his attention fixed exclusively on the case in which the moon must be forcing the reluctant water to oscillate fast enough for the tidal wave to keep up with herself, and thus the demonstration applies only to "shallow-"water tides. Consequently in *that* place the reader has only one side of the question set before him, and if he is unacquainted with the author's analytical treatment of the subject in the preceding part of the very paper now cited, or in the Art. referred to in our last Note, he may be (and apparently sometimes is) left under the impression that low water of the frictionless dynamical tide is necessarily always under the moon. But this geometrical proof can be easily applied, *mutatis mutandis*, to "deep-" water tides, if we remember that in them the moon must be forcing the water to oscillate slowly enough that the tidal wave, when produced, may keep back with herself.

Mem. It is easily seen that the equation for K, the height of the frictionless dynamical tide, given by Airy in p. 226 of the paper referred to in this NOTE, and in *Enc. Metr.* vol. v. p. 323 *,

$$K = -\frac{m\text{H}k}{i^2 - gkm^2}\cos(it - mx),$$

is the same as that (marked *A*) which we have copied in our last NOTE, although they look so different.

NOTE C, from p. 79.—It might, perhaps, seem at first sight that if the former relations of direction between the various movements of the water and the scheme of lunar forces were consistent with the moon's keeping up the tide, the reversal of these relations should be inconsistent therewith. But let us remember that in the former case the lunar forces were acting for only half their time concurrently with, and for the other half against, the movements of the water, just as with all ordinary oscillations or vibrations. We are no worse off now as regards this than we were before; the only difference is that

the concurrences and oppositions have exchanged their situations relatively to the moon.

Though it involves a little repetition, we may take this opportunity of putting together the answer to the following point, which some might possibly feel, at first sight, to be a difficulty. The spontaneous movement (if permitted) of the tide, when created, would be due to g; how then can the lunar tidal forces control that movement, if they be, as we have seen in p. 70, so excessively small in proportion to g? Because the lunar tidal forces act on and move all the water throughout its whole depth, and are sensibly independent of the existence of the tidal deformations; while the gravitation forces, though acting on all the water, are self-balanced as regards their pull on that below the level of low water; the forces which would produce the spontaneous movement of the tide, if free, consist only of the gravitation forces acting on the relatively very small superficial tidal protuberances, and are proportional thereto. Suppose that a wave like the tidal wave, and even of great magnitude, whether in "shallow" or in "deep" water, were created and started to move from E. to W. by some other agency, and then left to the moon alone; the confusion due to the continued baffling action between the independent lunar forces and the others, at first very much larger, would reduce the magnitude of the tidal deformation until the gravitation forces, dependent on, and proportional to, that deformation, became diminished enough to be under the control of the independent and constant lunar forces.

Note D, from p. 87.—This follows also from Airy's equation for the height K, above mean level, of the surface of the tidal wave with friction, at the angular eastward distance θ from the moon. See *Enc. Metr.* vol. v. pp. 331 * and 332 *. If the water be of less than the critical depth, the equation at the very bottom of p. 331 * may be written thus:—

$$K = \frac{-C}{(1+\tan^2 D)^{\frac{1}{2}}} \cos(2\theta + D);$$

C being a constant, and D an angle whose tangent is proportional to the coefficient of friction, which would be the same at every part of the tidal wave for any given friction, but would vary in the same direction as the friction (were this to alter); the upper limit of the magnitude of this angle being 90°.

It is high water when K is greatest, or cos $(2\theta + D)$ is at its minimum, that is when $2\theta + D = 180°$, or $\theta = 90° - \frac{1}{2}D$. If, now, the friction be so exceedingly great that D is nearly up to its maximum 90°, then θ (which without friction would be 90°) is slightly more than 45°. That is to say, the forward shift of the point of high water, due to exceedingly great friction, must be less than 45°.

If the water were of more than the critical depth, the equation would be

$$K = \frac{+C}{(1 + \tan^2 D)^{\frac{1}{2}}} \cos (2\theta - D);$$

from which it follows similarly that the backward shift of the point of high water, from exceedingly great friction, must be less than 45°.

These equations show also (what indeed is self-evident) that if the friction were very great, so that D was not far from 90°, making tan D very large, the magnitude of the tide would be exceedingly small.

N.B.—We omitted to explain that in Figs. 15 and 16 the arrows outside the ellipses represent the lunar tidal forces, the moon being to the right; those within the ellipses represent the gravitation forces of the disturbed water.

CHAPTER VI.

THE "HORIZONTAL" PENDULUM.

ALTHOUGH the moon's differential tidal force is quite easily calculable, and its magnitude perfectly well known, various attempts have been made to detect it by direct observation. The most important, but not the earliest, of these was carried out by a Committee, appointed for the purpose by the British Association, consisting of Professor George H. Darwin and others. The description of the apparatus used and of the experiments made therewith is given in the *Brit. Assoc. Report* for 1881. The attainment of the same object had been before sought by means of what is called the "Horizontal" Pendulum *.

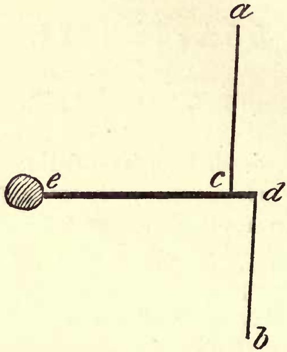
This is a simple contrivance intended for the measurement of very small horizontal attractions, and also for the detection of exceedingly small changes of level in the platform on which it stands. It is capable of very much greater sensibility, as regards the latter, than the most delicate spirit-level; moreover, its sensibility can be quite easily regulated in accordance with requirement.

Apparently the first to set up such an instrument was Hengeller, a pupil of Gruithuisen's at Munich, who, not later than 1832, did so in the manner shown in fig. 19. (See paper by Prof. Safarik in *Phil. Mag.* vol. xlvi., 1873, p. 412.) *de* is a rigid rod carrying at its end a ball of metal. The wire *ca* is

* This name is useful as a designation only, not as a description. The Pendulum's rod need not be horizontal, and its plane of oscillation *must not* be so, if it is to be a gravitation pendulum.

attached at one end to the rod, and at the other end to a point of support *a*; the wire *db* is attached at one end to the rod, and at the other end to a point of resistance *b*. The imaginary line joining *a* and *b* is nearly vertical, but leaning slightly

Fig. 19.



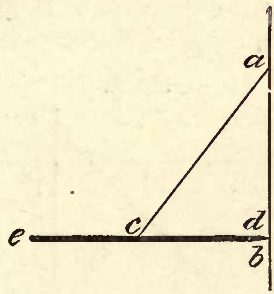
towards the pendulum. Of course the smallness of the distance *cd* does not contribute in the least to the sensibility of the instrument; it would do so only if the wires *ac* and *bd* were both always kept vertical. The sensibility depends only on the nearness of the axis *ab* to verticality. The horizontality of the pendulum-rod is of no importance, except for convenience. It might slope upwards or downwards from *d* at an angle of 45°, if desirable, without affecting the working of the Pendulum.

It has been stated that Gauss set up such an instrument. It is very likely that this is correct; but in the absence of description and of corroborative evidence, it is possible that the statement may be founded on a confusion between the bifilar pendulum now in question and Gauss's bifilar magnetometer, which, however, acts in a quite different manner.

About 1851, Mr. Alexander Gerard, of Gordon's Hospital (now College), Aberdeen, suspended such a pendulum in the manner represented in fig. 20. His account of it will be found in *Edinb. New Phil. Journ.* for April 1853. *de* is a rigid rod

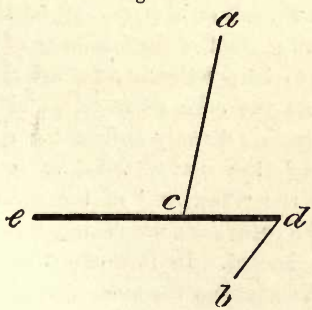
pointed at the end *d*, the point being of steel resting against an agate cup in *ad*, the side of a stiff standard. The thread *ac* is attached at one end to the rod at its centre of gravity and at the other end to the standard; *ad* should, of course, lean very slightly towards the pendulum.

Fig. 20.



In 1862, M. Perrot did the same, and exhibited his instrument to the French Academy. His mode of suspension, shown in fig. 21, was the same as Hengeller's, with, however, this difference, that the supporting threads *ac* and *bd* were acting very much less nearly against each other, the advantage of which is

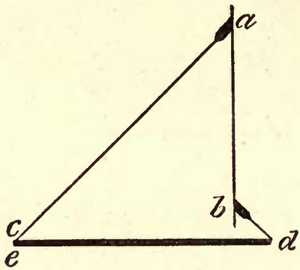
Fig. 21.



obvious. We shall return to this subject. Of course *cd* is less than *ce*. Perrot's description is in *Comptes Rendus*, vol. liv., March 21, 1862, p. 728.

In the early part of the year 1869, Rev. M. H. Close, of Dublin, suspended such an instrument in the manner shown in Fig. 22. *dc* is the pendulum-rod, and *ac* and *bd* the supporting threads attached at *a* and *b* to a stiff standard leaning very slightly towards the pendulum. (See *Practical Physics*, by Prof. W. F. Barrett and Mr. W. Brown, London, 1892, p. 241.) Of course the threads, as also those of Gerard and Perrot, had better not be twisted threads, which are liable to be affected by

Fig. 22.

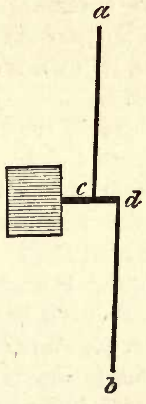


the hygrometrical state of the air. In this Fig. *ab* is an accurately straight and smooth edge projecting from the supporting standard towards the spectator, across which edge the separated silk fibres of the threads (which cannot be shown individually in the diagram, on account of the smallness of its scale) are bent at *a* and *b*, so as to be practically attached thereto. The fibres are made to cross the edge separately, and close together, so that each one is resting directly against the edge, in order that we may have simply the sum of what we may call (in analogy with "tension") the "flections" of the several fibres, without the tensions of the outer ones which would exist if they crossed the edge in a single cord. In this case there is no *torsion* of the supporting threads from the movement of the pendulum.

In the same year 1869, and, to judge from his own words, in the middle part of the year, Zöllner set up his well-known "Horizontal" Pendulum; his mode of suspension being the

same as Hengeller's, with, however, the difference that the heavy bob of the pendulum was placed quite near the axis of oscillation. See Fig. 23. His description of it will be found in a paper "On a new method for the Measurements of Attractive and Repulsive Forces," in the *Proceedings of the Royal Saxon Soc. of Sciences*, Nov. 27, 1869. He describes it also in *Phil. Mag.* vol. xliii. 1872, p. 491, giving a drawing of it in plate 3 of that volume*. The wires of Hengeller and the threads of Perrot are now thin watch-springs, each about 11 inches long and attached above and below, respectively, to an upright column, or standard, nearly two inches in diameter, supported on three feet and furnished with delicate levelling screws The

Fig. 23.



whole height of the stand being about 32 inches. The cylindrical bob, made of lead, and of about six pounds weight, carried in front a mirror by which readings were made on a reflected scale, according to a general modern practice. This pendulum is superior to Hengeller's, in that, for a given weight of the whole pendulum, the stress on the supporting bands, or wires, and therefore their necessary thickness and unavoidable stiffness, is

* It is somewhat unfortunate that "Horizontal Pendulum" does not occur in the Index of that volume.

much less; and also in that the weight of the bob has, for a given angular departure of the pendulum from the position of rest, so much less sideward moment against the supporting structure, which is calculated to cause lateral yielding therein.

Lord Kelvin's device for attaining the same object as above is described in the *Brit. Assoc. Report* for 1881, p. 93, and better in the *Report* for 1893, p. 291; it is in reality a Horizontal Pendulum in disguise.

Dr. von Rebeur-Paschwitz's Horizontal Pendulum may be described as similar to Zöllner's; except that instead of working by the torsion of elastic bands it turns on pivots at *a* and *b*, Fig. 23, consisting of steel points in agate cups. It is described and figured in the *Report* just quoted for 1893, p. 305. This instrument has the great advantage of being free from the modifications arising from the elasticity of the supporting threads &c., but it has, like others, its own special disadvantages which need to be guarded against.

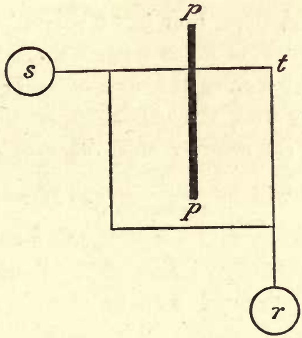
The Gray-Milne seismograph, suspended in 1891 by Prof. Gray, of Terre Haute, Indiana, U.S.A., and Prof. Milne, of Tokio, Japan, is a Horizontal Pendulum on the general plan of that represented in Fig. 20. See description and diagram of it in *Brit. Assoc. Report*, 1892, pp. 107–8.

Mr. Horace Darwin's Bifilar Pendulum, described and figured in the *Report* for 1893, p. 291, is a Horizontal Pendulum on the general plan of that in Fig. 22, above.

Of course such instruments are read, whenever practicable, by means of a scale reflected in a mirror attached to the pendulum. The stand should be supported on three points, say *r*, *s*, and *t*, Fig. 24, forming a right angle at *t*; there being levelling screws with graduated heads at *r* and *s*. If the pendulum be in the position *pp*, *r* would be the regulating-screw for determining the lean of the axis of vibration towards the pendulum, and so adjusting the sensibility of the instrument; and *s* would be the setting-screw for setting the pendulum to zero, when necessary, or for testing the sensibility.

It is very easy to see that, neglecting provisionally the force of torsion of the supporting threads, wires, &c., if the inclination, θ, of the axis ab to the vertical be very small, and the angular tilt to be measured be also very small (and the instrument has no special superiority without this), the delicacy of the instrument is proportional to $g/g \sin \theta$, or inversely proportional to $\sin \theta$, or to θ itself. For a given very small angular

Fig. 24.



change ϵ of the surface of the ground, transverse to the vertica plane of rest of the pendulum, the angular movement of the pendulum would be magnified to $\epsilon/\sin \theta$.

But the force of torsion of the supporting threads, or wires, &c., diminishes the magnification of the tilt to be measured and the sensibility of the instrument. For small departures of the pendulum from its position of rest, this force of torsion and the tangential component of $mg \sin \theta$ (m the pendulum's mass) vary sensibly according to the same law, viz., directly as the distance from the point of rest. Therefore the pendulum is oscillating, not merely under $mg \sin \theta$ acting at the centre of mass, but under $mg \sin \theta + \tau$; τ being the magnitude of the force of torsion of the supporting threads, or bands, as acting at the centre of mass of the pendulum, or the moment of torsion

divided by the distance of the centre of mass from the axis of oscillation. Consequently if the stand be tilted at right angles to the vertical plane of rest of the pendulum by the (small) angle ϵ, the angular movement of the pendulum would be to ϵ, not as mg to $mg\sin\theta$, but as mg to $mg\sin\theta+\tau$; that is, the angular movement of the pendulum would be multiplied by $\frac{mg}{mg\sin\theta\quad\tau}$; which fraction now represents the sensibility of the instrument. This cannot be increased above $\frac{mg}{\tau}$ unless by making $mg\sin\theta$ negative, that is, by very slightly *inverting* the pendulum, so to speak, as far as regards the action of gravitation upon it; that is, by making the axis of oscillation lean slightly backwards or away from the pendulum, so that it will oscillate under $\tau-mg\sin\theta$ (of course τ must be greater than $mg\sin\theta$), and its sensibility will be $\frac{mg}{\tau-mg\sin\theta}$. If τ could be absolutely unaffected by viscosity and constant, this would afford a means of increasing the sensibility indefinitely. But the force of torsion or of flection is interfered with by viscosity, and the present behaviour of a spring depends on its recent history as to temperature and strain. Consequently, when $\tau-mg\sin\theta$ is exceedingly small and the sensibility of the pendulum correspondingly great, the imperfection of the elasticity will become important, and the condition of the pendulum might be, only for a comparatively short time, that of stable equilibrium.

If we may surmise from the behaviour of Zöllner's Pendulum on the occasion described in p. 494 of the vol. of *Phil. Mag.* above referred to, the pendulum was thus inverted; though we cannot be quite sure of this, without knowing the moment of torsion of the watch-springs and the moment of inertia of the pendulum.

Therefore, since it is obviously desirable that the "Horizontal Pendulum" should be, as nearly as possible, a pure gravitation pendulum, and that its action should depend as little as possible on the force of torsion or of flection, of the supporting threads,

wires, or bands, these should be as thin as may be practicable; and, to allow of this, they should be made to support the pendulum in such a way as to have as little stress on them as possible. Now, judging from the drawing of Zöllner's Pendulum, supposing it to be drawn to scale, the mean stress on each of the watch-springs is at least 3·5 times the weight of the pendulum; they must be strong enough and thick enough to withstand such a stress, and therefore they must have an undesirably great force of torsion, to interfere, in the way we have noted, with the performance of the Pendulum (this objection is much stronger against Hengeller's Pendulum). To this we may add that for certain reasons it is desirable that the gravitation zero and the torsion zero should coincide as nearly as may be; but when the force of torsion is greater than may be avoided, the disadvantage of the non-coincidence of the zeroes is so likewise. (See NOTE A.)

It would seem, then, that the mode of suspension illustrated in Fig. 22 is preferable to some of the others now described. The stress on each thread is less than three fourths of the weight of the pendulum; each thread, therefore, need only be strong enough and thick enough to endure that stress with safety.

If the edge were cylindrical like the side of a very fine needle, the magnitude of the flection, during an angular movement of the pendulum, would evidently be constant, and not, as above, proportional to the pendulum's angular distance from any particular point. In this case the flection would not diminish the sensibility of the instrument; but merely alter very slightly its zero point, or position of rest.

In order to guard against sagging in the support of the pendulum this should be not only as short as is consistent with other requirements, but solid and strong; and the weight of the pendulum should be kept down as much as may be, and stops should be provided to prevent too great departure from the position of rest. It should of course be contained in a case or box, proof against movements of the air, and with sufficient non-conductivity of heat; the inside of the box being lined with

tin-foil, with sufficient metallic connection with the ground, to guard against unequal distribution of electricity; and the pendulum itself should be made of the least magnetic or (so-called) diamagnetic substance.

The sensibility of the instrument, or the ratio of the angular movement of the pendulum to the angular tilt to be measured, might, perhaps, be obtained approximately by an exceedingly delicate levelling screw, or by a screw working with a differential action, which could be depended upon to produce a known very small lateral tilt in the stand of the pendulum, to be compared with the consequent angular movement of the pendulum.

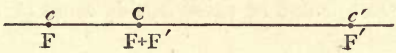
But the following would doubtless be a much better way of obtaining the sensibility, if the effect of the resistance of the air were quite negligible. The sensibility is, as we have said, $\frac{mg}{mg \sin \theta + \tau}$, or the ratio of the forces acting at the centre of mass when the pendulum is hanging freely and when suspended in the manner now in view; both forces being proportional to the angular distance of the pendulum from its position of rest. But these are inversely proportional to the squares of the corresponding times t_1 and t_2 of oscillation. Let the shape of the pendulum be such that its radius of oscillation can be easily obtained from measurements of its parts; this will give the time t_1. The time t_2 is known from direct observation; thus t_2^2/t_1^2, the sensibility of the pendulum, is known. If the resistance of the air be proportional to the velocity of the pendulum in its swing, which it is very approximately for very small velocities, its interference with the isochronism will be exceedingly small; but it will very slightly increase the period of oscillation, and so make the sensibility calculated in this manner very slightly greater than the truth.

There seems to be very little likelihood that the moon's tidal force will ever be measured by the Horizontal Pendulum, or by any instrument working as a level does. The experimenter must first make, with Archimedes, the rather important request:

"Give me whereon I may stand." Not to dwell on movements in the earth's crust, disturbances of level by changes of temperature, the moon's own tidal deformation of the body of the earth, &c., it would appear that in most cases, at least, a gentle breeze pressing on the side of a house would make the whole basement floor tilt to leeward through an angle considerably greater than the greatest change in the vertical by the moon's tidal force.

NOTE A, from p. 101.—Let c and c', Fig. 25, be two centres of force varying directly as the distance; the absolute value of the forces, or their magnitude at unit distance, being F and F′,

Fig. 25.



respectively. It is easily seen that they are equivalent to a force having the same law, with absolute magnitude $F+F'$, and with centre C whose distances from c and c' are inversely as F and F′. (This obtains, of course, not only in the line cc', but throughout all the space around C.)

It is evident that if the zeroes in the text do not coincide, and if the instrument be tilted slightly in the vertical plane of rest of the pendulum, there will be a horizontal movement of the pendulum, which might be taken as an indication of a lateral tilt.

Mem. We are indebted to Mr. Charles Davison, Secretary of the British Association's Committee on Earth Tremors, for some information on the subject of this Chapter.

CHAPTER VII.

THE MOON'S VARIATION.

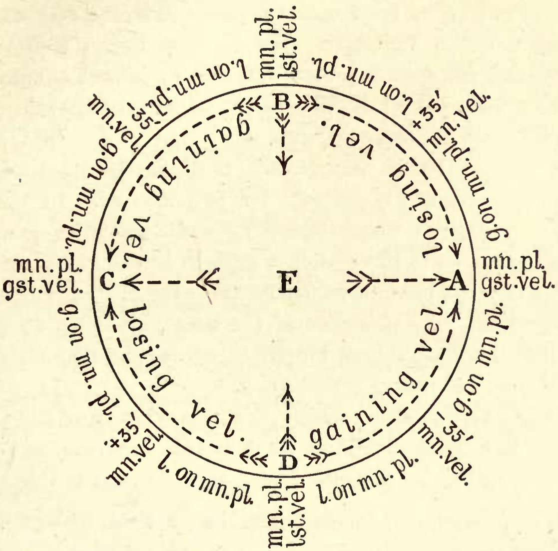
The space that can be afforded, in ordinary elementary treatises on astronomy, to the moon's Variation and Parallactic Inequality is necessarily small; so that various important and interesting matters connected with those lunar inequalities must be left out of consideration. But as our space is at our own disposal, we can now give to some of these details more of the attention which they deserve.

We assume that the reader is acquainted with the nature and the general cause of the inequality of the moon's motion called the Variation. It is produced by solar differential forces, tangential and radial, similar to those which produce the tides. Fig. 26 is a diagram of the chief particulars of the Variation scheme of perturbing forces, and of the changes of the moon's motion: E is the position of the earth; ABCD is the moon's orbit round the earth; the moon revolving in the direction indicated by the order of those letters. The sun is supposed to be away to the right, over A, at a distance from E representing 388 times EA, the moon's distance from the earth. The tangential arrows and the radial ones drawn with broken lines show the reaches or ranges of action, and the directions, of the solar disturbing forces, tangential and radial. The contractions will be readily understood: *mn*, mean; *gst*, "greatest"; *lst*, "least"; *pl*, "place"; *vel*, "velocity"; *g*, "gaining"; *l*, "losing." The terrestrial tangential forces, to be mentioned further on, are not represented from want of room; but this is of no consequence, if it be remembered that in the

Variation scheme of forces they always agree with the solar tangential forces, both as to reach, or range, and direction.

The disturbing forces are calculated, *mutatis mutandis*, precisely

Fig. 26.



as the tidal ones. If the earth's mean attraction on the moon be taken as unity, then for a circular lunar orbit, the tangential disturbing force will be $\frac{3SR^3}{2ED^3}\sin 2\epsilon$, or $\frac{1}{120}\sin 2\epsilon$, and the radial force $\frac{3SR^3}{2ED^3}(\cos 2\epsilon + \frac{1}{3})$; S being the sun's mass, E the earth's mass, D the distance of the sun from the earth, R the distance of the moon from the earth, and ϵ the moon's elongation from the sun reckoned from conjunction, eastwards, right round to 360° *. The maximum value of the solar tangential force,

* These expressions show that the Variation forces are, *quam prox.*, inversely proportional to the cube of the sun's distance. It might seem, at

which occurs at the octants, is 1/120th of the earth's mean attraction on the moon, that of the radial force outwards at syzygies is 1/90th of the same; its inwardly directed maximum, at quadratures, is half this, or 1/180th. These forces are, then, very small relatively to the earth's attraction on the moon; which circumstance is of much importance in the mathematical discussion of the Variation. Let us note that the tangential forces vanish when $\sin 2\epsilon = 0$; that is at syzygies and quadratures, and the radial ones when $\cos 2\epsilon = -\frac{1}{3}$; that is at the four points distant by 54° 44′ from syzygies.

The inequalities in the moon's motion, due to these differential forces, are important, for two reasons. In the first place, they present some interesting and instructive problems, both dynamical and kinematical; and, in the second place, it is of great moment to know approximately enough their magnitude, for the construction of tables of the moon, by which to be able to predict the moon's true longitude, or angular distance on the ecliptic from the first point of Aries.

When the latter object is in view, the equation of the moon's angular Variation, or difference between her true and mean longitude, caused by the said forces, is usually given thus—

$$\text{M's tr. long.} = \text{her mn. do.} + C \sin 2\,(\text{M's mn. long.} - \text{S's do.}), \qquad (1)$$

M being the moon, S the sun, and C the coefficient of the Variation in longitude. (See NOTE A.)

But our present main object is to consider the matter simply on its own account; and as the disturbing forces are connected with the moon's *elongation*, or angular distance from the sun, it will be simpler and more interesting to consider their effects on this, rather than on her longitude, to which we are now

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first sight, that they are directly proportional to the cube of R, the moon's distance. But they are proportional only to the first power thereof. The R^3 comes in on account of the earth's mean attraction on the moon being taken here, for convenience, as unity.

indifferent. Moreover, we are now concerned with the Variation only in the abstract, or in its purity, as we may express it. We shall therefore take the moon's undisturbed orbit and the sun's relative, or apparent, annual orbit round the earth as both circular, and in the same plane, and the angular velocities of both luminaries in those orbits as constant; *so that the sun's true, and mean, longitude will be the same.* Subtracting, then, the sun's true longitude from the left side of the above equation, and his (now) equal mean longitude from the other side, and adopting what seems the best value of the coefficient C, we have (from Hansen) the equation

$$\epsilon = \phi + 35'\ 45''\sin 2\phi\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which ϵ is the moon's true elongation from the sun, and ϕ her mean elongation, for the same instant of time, both reckoned eastwards from the sun up to 360°. Thus, then, the moon's pure abstract Variation in elongation, or her departure from ϕ, is $+35'\ 45''\sin 2\phi$.

Now let r be the moon's actual radius-vector, or distance from the earth, and R her mean distance, and we shall have (from Hansen)

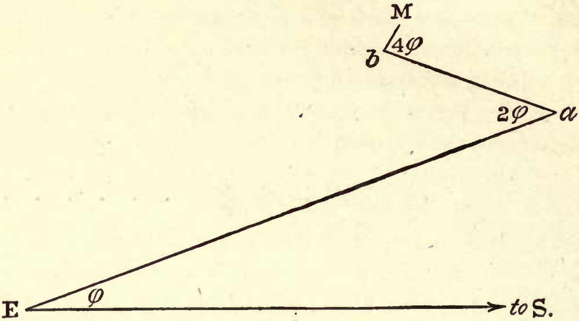
$$r = \mathrm{R}(1 - \tfrac{1}{136}\cos 2\phi). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The Variation in the moon's distance from the earth is, then, $-\frac{1}{136}\mathrm{R}\cos 2\phi$.

From these equations (2) and (3) in combination may be easily derived a simple geometrical construction for obtaining the moon's position in space for any assumed ϕ, or mean elongation. First let us note that the coefficient $35'\ 45''$ in equation (2) is, in circular measure, 1/96. Whence the moon's linear departure, forwards or backwards, from the line of her mean radius-vector R is $+\frac{1}{96}\mathrm{R}\sin 2\phi$, *q.pr.*; her departure from her mean distance being, as we have seen, from equation (3) $-\frac{1}{136}\mathrm{R}\cos 2\phi$.

In Fig. 27, E is the place of the earth, ES the direction of the sun. The construction is as follows:—Draw Ea to represent, in magnitude and position, the moon's mean radius-vector, at a given time, whose length is 238,820 miles; SEa is ϕ, and a is

Fig. 27.



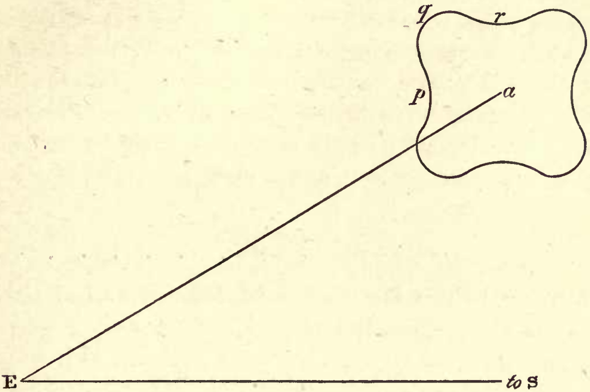
the moon's mean place. Draw ab making the angle 2ϕ with Ea (or ϕ with ES), to represent the length $\frac{1}{2}R(\frac{1}{96}+\frac{1}{136})$, or 2093 miles, then draw bM making the angle 4ϕ with ab (or 3ϕ with ES), to represent the length $\frac{1}{2}R(\frac{1}{96}-\frac{1}{136})$, or 337 miles; then M is the moon's true place for assumed ϕ. (See Note B.) With the exception of the line Ea, which is necessarily vastly too short, this Fig. and Figs. 30 and 31 are drawn to scale; the scale being the same in all.

Thus we see that the moon's pure Variation orbit*, according to equations (2) and (3), is a compound epicyclic curve as referred to ES regarded as fixed. Ea is the radius of the deferent circle turning progressively with the moon's mean angular velocity; ab is the radius of the first epicycle turning retrogressively with the same angular velocity; bM is the radius of the second epicycle turning progressively with thrice the said angular velocity.

* By "Variation orbit" we mean the moon's orbit as deformed by the Var. disturbing forces alone; the undisturbed orbit being supposed circular.

The curve described by the moon about her mean position is, of course, a simple epicyclic, relatively to ES regarded as fixed, whose deferent is the first epicycle above mentioned. It is a four-lobed curve, like that in Fig. 28. Its greatest diameters

Fig. 28.



being $\frac{1}{96}$R, and the least $\frac{1}{136}$R. This curve, as its centre is carried round on the end of Ea, or R, always keeps the same shortest diameter parallel to ES, and so preserves the same aspect towards the sun. The moon describes this curve about her mean place once in a synodical month and retrogressively. When in conjunction she is at p in the curve; when in first octant she is at q; when in first quadrature at r, &c.*

The movement of the moon in her Variation orbit can be represented in another manner, which is of considerable interest. It follows directly from the same equations (2) and (3). (See NOTE C.)

It is often said simply that the moon's Variation in elon-

* Of course relatively to fixed space this curve itself rotates once in a year progressively; but we are not now concerned with that.

gation from the sun is proportional to the sine of twice her elongation, that is to $\sin 2\epsilon$. The discrepancy involved is evidently very small. It can be seen without difficulty that if in equation (2) we substitute $\sin 2\epsilon$ for $\sin 2\phi$, it will make a difference in the angular Variation of only $-\frac{1}{96}35' \, 45'' \sin 4\phi$. This vanishes at syzygies, quadratures, and octants. It is at its maximum value, alternately negative and positive, at the eight points halfway between those just mentioned; but this maximum does not amount to $\cdot 23''$. We may, then, take the liberty of writing equation (2) in the following form, which is more convenient, while always fully accurate enough for our present purpose, and quite accurate at the eight points just mentioned; viz.

$$\epsilon = \phi + 35' \, 45'' \sin 2\epsilon. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

According to this the moon is at her undisturbed place in elongation at syzygies and at quadratures, most before that place when 45° past syzygies, and most behind it when 45° past quadratures.

If we substitute, in equation (3), $\cos 2\epsilon$ for $\cos 2\phi$, similarly to what we have done with equation (2), this will involve a discrepancy proportional to $-\sin^2 2\phi$, which varies from zero, at syzygies and quadratures, to its maximum, always negative, at octants; but this maximum is only 1/48th part of the greatest value of the Variation in the moon's radius-vector. When written thus—

$$r = \mathrm{R}(1 - \tfrac{1}{136} \cos 2\epsilon) = \mathrm{R}(1 - C \cos 2\epsilon), \quad . \quad . \quad . \quad (5)$$

it becomes a polar equation of the moon's Variation orbit; which is quite sufficiently accurate for our present purpose. The pole, of course, is at the earth, and the curve is referred to the line ES as its prime axis, or prime vector; as this revolves once in a year the Variation oval does so likewise, along with it.

According to this equation the moon's orbit, if subject to no other inequalities than those of the Variation, would be an oval

with its shortest axis in the line of syzygies, or directed towards the sun, and its longest axis in the line of quadratures; these axes being to each other as $1-\frac{1}{136}$ to $1+\frac{1}{136}$, or as 67 to 68. The proportion given by Newton was very close to this, viz., 69 to 70.

Owing to the smallness of the coefficient $\frac{1}{136}$, equation (5) differs practically but little from that of an ellipse. It gives a curve which is slightly flatter at syzygies and quadratures (where it coincides with (3)) than an ellipse with the same principal axes. The radii of curvature at those points can be easily obtained by the geometrical method; they are, for syzygies, $R\frac{(1-C)^2}{1-5C}$, and for quadratures, $R\frac{(1+C)^2}{1+5C}$; C being the coefficient $\frac{1}{136}$. (See NOTE D.)

The moon's velocity is greatest at syzygies, least at quadratures, and at its mean at octants. But we shall return to this.

There are some very interesting particulars, both kinematical and kinetical, connected with the Variation, which, being contrary to what many persons would expect beforehand, present to them, at first sight, the appearance of paradox.

One of these is that, as we have just seen, the Variation orbit should have its shortest axis directed towards the sun. The dynamical reason for this is given geometrically in Newton's *Principia*, Book III.; but of course the analytical treatment of the question is more powerful and complete. It is most respectfully submitted that the ordinary short popular "proof" of this is quite inadequate, for more than one reason. May we venture, while deprecating the imputation of rashness, to propose another proof, as we hope it to be. In excuse for its length we beg to plead that no proof can be sufficient unless it takes into account all the principal elements of the problem; that is to say, not only the tangential, but the radial, disturbing forces, as well, and also the condition under which

they act, viz., the law of the earth's gravitation attraction on the moon.

Elementary Proof of the character of the Variation inequalities in Elongation and Radius-Vector.—It is probably impossible to give an entirely *à priori* proof of this which shall be both simple and quite complete. Some of the following sketch-argument depends for its ready applicability on the fact of the smallness of the perturbing forces and of the perturbations with which we are now engaged. We know, *à priori* (see p. 106), that, relatively to the earth's attraction on the moon, the Variation perturbing forces are very small; and we know from observation that the resulting perturbations are so likewise. We can, then, consider the actions of the tangential and of the radial forces separately, and can combine, by simple superposition, their several effects; these being comparable to different sets of "small oscillations." Supposing still, for simplicity, the moon's undisturbed orbit and the sun's relative orbit round the earth to be both circular, since we know, *à priori*, that the scheme of the Variation disturbing forces is symmetrical on each side of the line of syzygies *and also* on each side of that of quadratures, and, by experience, that the deformed orbit is stable, we are justified in concluding that that orbit must be itself symmetrical on both sides of each of those lines, as principal axes.

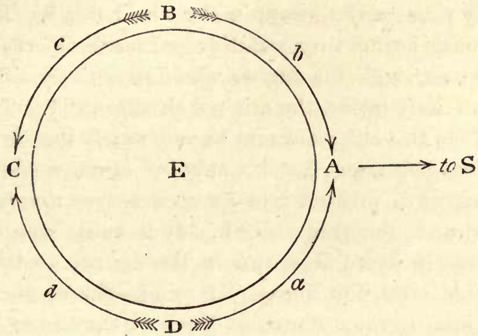
In Fig. 29 the circle represents the moon's undisturbed orbit supposed circular; the sun being over A, and the moon revolving in the direction ABCD.

We shall neglect, for the present, the sun's relative annual revolution round the earth; the result of which, as we shall see, is merely to increase the effects now to be considered.

We shall take first the solar tangential disturbing forces considered by themselves, and as acting on an originally circular orbit of the moon round the earth. We shall first suppose the sun's disturbing power to begin to exist when the moon is passing D. We are, however, in a little difficulty here. The

sun never began to act at any particular point on a previously undisturbed lunar orbit. If we begin at D with force *a*, as we shall call it, we must compound its effect with that of its successor, which we have marked *b*; but we have no more right to do this than to compound its effect with that of its predecessor *d*.

Fig. 29.



In order to approximate to the right thing, we must take different starting-points in succession, and then combine the results. It will be simplest to take the four points of syzygy and of quadrature, in succession, as starting-points. We shall begin then at D, with tangential force *a*. It is, of course, the *impulse* of *a*, or the integral of its product with its time of acting, with which we have to do. Now the *force a* is a maximum at the octant, which is the middle of its reach, or range, DA, and its magnitude, which is, as we know, very small even at the maximum, is always, as we see from its formula, p. 105, equal at equal distances on each side of its maximum, and zero at both ends of its reach DA; so that the force is exceedingly small for some length towards each end of its reach. It will, therefore, involve a very small inaccuracy, as relates to our present subject, if we regard the whole impulse as condensed into a very short-lived impulse of the same magnitude, like an impact, acting tangentially at the octant. We can treat similarly the opposing

tangential impulse *b*. Now, in consequence of the law of the earth's attraction, which would make the moon move in a focal ellipse round the earth, the impulse *a*, by itself, would produce an elliptical lunar orbit with an apogee 180° distant, at the octant under the letter *c*. But the opposing tangential impulse *b*, which is equal to *a*, would, if it acted by itself on the still undisturbed moon, cause her to describe an ellipse with its apogee very near to the octant under the letter *b*. These two ellipses would have a very small proportional difference as to magnitude; although the former would be entirely outside, and the latter entirely inside, the original circular orbit. The eccentricities of the two ellipses would be very nearly the same. Now these two apogees under *b* and *c*, only 90° apart, would combine or coalesce, as is evident, into an apogee very nearly halfway between, close to the quadrature B. It is easily seen that this apogee would be above the circle in the figure; as the former ellipse would cross the line of EB at a height above the circle due, *inter alia*, to the distance of three octants from the apse under the letter *a*; while the other ellipse would cross EB below at a depth due to the distance of only one octant from its apse under *b*. The composition of the greater rise with the smaller fall would give a crossing of the line of EB above the circle. Thus the impulses *a* and *b* acting together, apart from the others, would produce an apogee very near B above the circle. Now let us start at A with impulse *b*. We shall find, in a corresponding manner, that impulses *b* and *c*, acting together apart from any others, would produce a perigee very close to C, and below the circle; and that *c* and *d*, similarly, would produce an apogee near D above the circle; and *d* and *a* together a perigee near A below the circle. But we must not use each tangential impulse twice over; therefore, to avoid this, we must take only half of each in our successive stages round the lunar orbit. Thus the tangential disturbing forces alone would deform the moon's originally circular orbit into an oval with its longest axis in quadratures *.

* We see here that though the tendency of the immediate local action of the tangential force in the quadrant DA would be to make the moon rise

This deformation of the orbit by the tangential disturbing forces gives rise, of course, to tangential components of the earth's attraction on the moon, or, as we shall call them, terrestrial tangential forces, whose positions and directions are, in the case of the Variation, the same as the solar ones. These, therefore, are auxiliary and increase the inequalities of the moon's rate of revolution round the earth. Thus the solar tangential forces are the means of causing considerably greater inequalities in the moon's velocity, both linear and angular, than they could produce by their immediate local action.

Therefore, if the tangential disturbing forces were to act by themselves, apart from the radial ones, the moon's linear velocity would be greatest at syzygies and least at quadratures; and this would be so, *à fortiori*, as regards her angular velocity. But we cannot assume, at once, that these things must be actually so; for these forces do not act by themselves. If it should so happen that the radial forces by themselves would produce a sufficiently greater elongation of the orbit in syzygies, the terrestrial tangential forces created thereby, which would then be oppositely directed to the solar ones, might be the greater of the two; so that the moon's velocity might be least at syzygies, and greatest at quadratures.

We therefore turn now to the radial disturbing forces; first taking by themselves the outwardly directed ones, whose action extends for 54° 44′ on each side of both syzygies. It will be seen, on a very little consideration, that these by themselves would produce a lengthening of the radius-vector at some angular distance after syzygy; because their effect in increasing the moon's distance from the earth must evidently continue for some time after they have ceased to act with their greatest efficiency, which happens at syzygy. We shall see in NOTE E that in

from the earth, yet in consequence of the whole general action even of the tangential disturbing forces, alone, the moon is really falling earthward all through that quadrant. This illustrates a principle to which we shall refer again.

consequence of the law of the earth's attraction, the greatest lengthening, or apogee, will be exceedingly near to quadrature. Similarly, of course, with the (outward) radial forces about opposition.

Now the inwardly directed radial forces, extending for 35° 16′ on each side of both quadratures, would, by themselves, tend to produce perigees very near to both syzygies. But the latter forces, or, we should say, the *impulses* of which they are a factor, are roughly of only one third the value of the outward impulses; since the average magnitude of the *forces* is only about one half, and their time of acting only about two thirds, those of the former (see NOTE F); and therefore the result of their action is much smaller than that of the outwardly directed forces; but it is auxiliary as regards the general effect now in question. This effect, as with that of the tangential forces, although dependent for its existence on the law of the earth's attraction, is controlled thereby, so that it cannot exceed a certain magnitude. Thus the radial disturbing forces, by themselves, would produce a deformation of the moon's orbit similar in general character to that due to the tangential ones; and therefore would by themselves give rise to tangential components of the earth's attraction on the moon, resulting in inequalities in the moon's velocity, both linear and angular, similar to those produced by the tangential forces (but much smaller) *.

We *now* know that we actually have maximum linear velocities of the moon at the ends of the (shorter) syzygy axis, and minimum velocities at the ends of the (longer) quadrature axis; and that this is true *à fortiori* of the angular velocities.

* We see that we must not institute too close a comparison between the formation of the oval of the dynamical tides and that of the Variation orbit. Though the system of the lunar differential forces producing the tides and that of the solar ones producing the Variation are precisely similar, yet they are acting under very different conditions. Though the Variation oval is necessarily placed with its side towards the disturbing celestial body, the tidal one is so placed only under the special circumstance that the water is of less than a certain depth.

There are, therefore, two reasons for expecting that the Variation orbit must be flatter at syzygies, and more curved at quadratures, than elsewhere. But probably this cannot be proved by any simple considerations such as those to which we are now confining ourselves; and as there are various particulars connected with the Variation which turn out to be contrary to what a large proportion of reasonable persons would anticipate, this might be one of them; and it is, relatively to what we know so far, quite possible that this natural expectation might prove erroneous. Since the moon is nearest to the earth at syzygies, it might very well happen that the consequent increase of the earth's attraction there might be greater than the sun's outward disturbing force at those points (we shall see indeed further on that this is actually so), and therefore the possibly greater earthward force at syzygies might overcome the effect of the moon's greater velocity at those points, as regards the curvature of the orbit there; and correspondingly at quadratures. This much, however, is quite clear, viz., that the Var. orbit is less curved at syzygies than the equidistant circle there, and more curved at quadratures than the greater equidistant circle *there.* But this will not prove the matter in question; because, for all we could yet say to the contrary, the orbit might be four-lobed.

We have already alluded to the fact that the relative annual revolution of the sun round the earth increases the lunar inequalities now in question; this it does by giving to the system of alternating disturbing forces a longer period, viz., half a synodical month, than what they would have without it, viz., half a sidereal month.

The disturbing forces, then, have the effects now mentioned in consequence of the condition that the earth's attraction on the moon is inversely proportional to the square of the distance; so that that attraction is always endeavouring to make the disturbed lunar orbit an ellipse with the earth in one focus. But this same condition, which determines the character of those

effects, determines also the limit of their magnitude under the action of those disturbing forces. The earth's attraction, because of the law of its action, keeps down the height of the apogee at each quadrature by endeavouring to make there a perigee answering to the apogee of the preceding quadrature. The incompatibility between the central oval, due to the disturbing forces, and the focal ellipse, that the earth's attraction is always trying to produce in the moon's disturbed orbit, affords the earth an opportunity and power of reluctation against the deformation, which would be greater than it is if the deformation were so. The sensibly constant disturbing forces are able to produce, by accumulation, only that amount of deformation at which further increase would become intolerable, and at which the earth acquires sufficient power of reluctation and control to balance the action of those forces.

The opposition, in this respect, between the solar and the terrestrial forces is due to the fact that the scheme of disturbing forces and the consequent Var. orbit are symmetrical on each side of two rectangular axes passing through the earth; while the elliptical lunar orbit, which the earth's attraction is always endeavouring to produce, would have only one axis of symmetry passing through the earth. There is, in this respect, an important and interesting difference between the Variation and the Parallactic Inequality, to which we shall return in the next chapter. (See NOTE G.)

We come now to another apparent paradox, already alluded to, connected with the Variation, which it is particularly necessary to notice as it is so generally overlooked, sometimes with inconvenient results. It is this, that the effect of the terrestrial tangential forces in producing the moon's Variation in longitude is considerably greater than the direct effect of the solar ones. This can be seen as follows—

We know already that, the earth's mean attraction on the moon being taken as unity, the solar tangential force is $\frac{1}{120}\sin 2\epsilon$;

but, accepting equation (4) as the polar equation of the Var. orbit, which it is, *quam prox.*, it is not difficult to find that the terrestrial tangential force is $\frac{1}{68}$ sin 2ϵ $(1+\frac{1}{45}\cos 2\epsilon)$, *quam prox.* (See NOTE H.) Therefore the two forces, practically speaking, vary very nearly according to the same law, viz., as sin 2ϵ; and the terrestrial tangential force is to the solar, at any given point of the Variation orbit, in a ratio never less than 120 to 68; or say 7 to 4, very nearly. But the shares of the moon's whole displacement in elongation produced by these two forces at any given point are very approximately proportional to the respective magnitudes of the forces *. Those shares are, therefore, to each other very nearly in the said proportion of 7 to 4.

Thus we see that the solar disturbing forces produce the inequalities in the moon's velocity in various places much more by means of their deformation of the lunar orbit than by their direct immediate influence on the moon's velocity near those places.

If the general action of the solar disturbing forces had, by accumulation of effects, changed the assumed originally circular orbit into its present shape, but with the longest axis directed to the sun, as it might have done, for all that we could tell beforehand to the contrary, and as most people would expect it to do, the solar tangential forces, while still very nearly indeed of their present magnitude, would retain, of course, their present directions; but the terrestrial tangential forces, while still almost precisely of their present magnitude, would be reversed in direction. The terrestrial would actually overpower the solar tangential forces, as regards their immediate effect on the moon's velocity; and the result would be that notwithstanding the acceleration due to the solar tangential forces, the moon would go gradually slower in the quadrant DA, and, for a

* This is so; but only because the two forces vary so very nearly according to the same law, and because the changes of velocity due to each of the forces are so exceedingly small relatively to the mean velocity of the moon in her orbit round the earth.

corresponding reason, gradually faster in the quadrant AB; and so on.

It must not be thought an absurdity to contemplate beforehand the possibility of such an action ever taking place; for we shall find an instance of it farther on in the Parallactic Inequality orbit.

Therefore we cannot lay down that the moon must be necessarily quickening or slackening her pace, according as the solar tangential forces are acting with or against her motion, until we *first* know enough respecting the deformation of the Variation orbit and the position of its greatest and least axes.

If the attention be fixed too strongly on the immediate, direct action of the tangential and the radial disturbing forces, it leads naturally to the over-statement frequently met with, viz., that the Var. in longitude is almost entirely due to the tangential disturbing forces. This would undoubtedly be so if the Var. depended principally on the direct action of the two disturbing forces; but we have seen that such is by no means the case. The Var. in longitude is, as above stated, due principally to the deformation of the moon's orbit, in the production of which deformation the radial disturbing forces have a share rather greater than that of the tangential forces. The tangential disturbing forces are, indeed, more important than the radial ones in causing the Var. in longitude; but the share of that inequality to be assigned to them is not as much as double that of the radial forces (see Godfray's *Lunar Theory*, p. 88).

To return to a matter alluded to above:—It might be supposed that since the radial disturbing force, which is directed away from the earth at syzygies, is at its maximum at those points, therefore the whole earthward pull on the moon is least at those points, and, for a corresponding reason, greatest at quadratures. Airy, in *Gravitation*, p. 66, shows that he was aware of the erroneousness of this; but, interestingly enough, we find that when giving his admirable lectures on astronomy at Ipswich (entitled *Popular Astronomy*), he had forgotten his own know-

ledge of its incorrectness. We can easily see for ourselves how the matter stands. The earth's mean attraction on the moon being taken as unity, the solar radial force at syzygies is, as we have seen, 1/90, which is to be deducted from the earth's attraction at syzygy; but, on the other hand, since the moon's distance from the earth at syzygy is less than the mean by 1/136th, the earth's attraction on the moon, which varies inversely as the square of the distance, is at those points greater than the mean by 2/136ths, or 1/68th. Therefore the whole earthward pull on the moon at syzygies is $1-\frac{1}{90}+\frac{1}{68}$, which is greater than unity, the mean, and (as we can easily see) a maximum. Similarly the whole earthward pull on the moon at quadratures is $1+\frac{1}{180}-\frac{1}{68}$, which is less than unity, the mean, and (as can be easily seen) a minimum.

The Var. forces being proportional to the inverse cube of the sun's distance, it might seem reasonable to believe that the moon's Var. in elongation must be proportional to the same. But in reality this lunar inequality is a very complex function of that distance, which would vary, not indeed very differently from the inverse cube thereof, but at a higher rate.

We may here refer to what some might regard, at first sight, as a kinematical paradox; though it does not belong specially to the Variation. Since the moon is being retarded, both by the solar and the terrestrial tangential forces, while passing from A to B, it might be thought that she must be behind her mean place in that quadrant. But it must be remembered that while she enters on that quadrant at her mean place, she is then moving with her greatest velocity; and as long as her velocity is above the mean, she is gaining on her mean place which she had at A; though her velocity be in the act of diminishing down to the mean under both the opposing tangential forces. Similarly in the next quadrant, though she is being accelerated there by both the tangential forces, she is losing on her mean place; because, while entering on that quadrant at her mean place, she

is moving with her least velocity; and she must continue to lose on her mean place until her velocity has been increased by the tangential forces up to the mean. When at her mean place, as at syzygies and quadratures, she is moving with greatest or least velocity, respectively; because she has then been, for the longest time, undergoing acceleration or retardation, respectively. When most before or behind her mean place, as at the octants, she is moving with mean velocity; because she has then just ceased to gain or to lose, respectively, on her mean place.

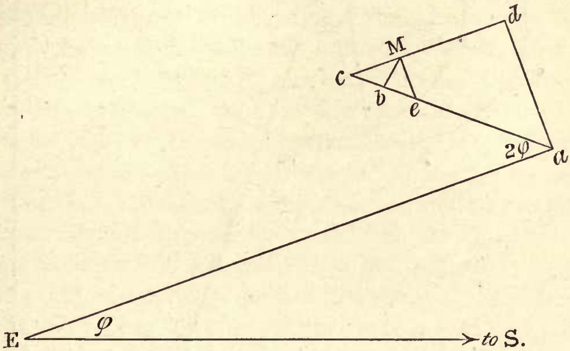
Taking these considerations in connection with the results of an inspection of equation (4), we see that we can fill in, for ourselves, all the writing in Fig. 26, the diagram of the Variation.

NOTE A, from p. 106.—It is evident that equation (1), by itself, cannot give the accurate value of the effects of the Var. forces whose magnitudes always depend on the moon's true elongation from the sun, or the difference between the moon's and the sun's true longitudes. But it is intended only as a first step towards obtaining the moon's Variation in longitude, which must be supplemented by other much smaller equations of the moon's motions connected with this inequality. The angular distance described between the brackets in equation (1) has been given in several other ways; *e. g.* as the moon's equated long. *minus* the sun's true long., as the moon's mean long. *minus* the sun's true long., &c.; Laplace's form of it and Hansen's differ, not only from these, but (very slightly) from each other. All the differences are small and more or less completely made up for by subsidiary equations. It is only very approximately correct to say, as is often said, speaking roughly, that the Variation vanishes at syzygies and quadratures; this would be true only if the angular distance within the brackets were the moon's true, *minus* the sun's true, longitude.

NOTE B, from p. 108.—This can be seen as follows. (In Fig. 30, the points marked *a*, *b*, M, are the same as those similarly marked in Fig. 27; and neglecting the line E*a*, the

scale is the same in both Figs.) Ea being the moon's mean radius-vector, necessarily drawn vastly too short relatively to the other lines, draw ac making the angle 2ϕ with Ea, and of length to represent $\frac{1}{96}$R; then draw cd parallel to Ea and sensibly pointing backwards to the earth, draw ad at right angles to Ea and cd; then $ad = \frac{1}{96}\text{R}\sin 2\phi$. Take e so that ae may

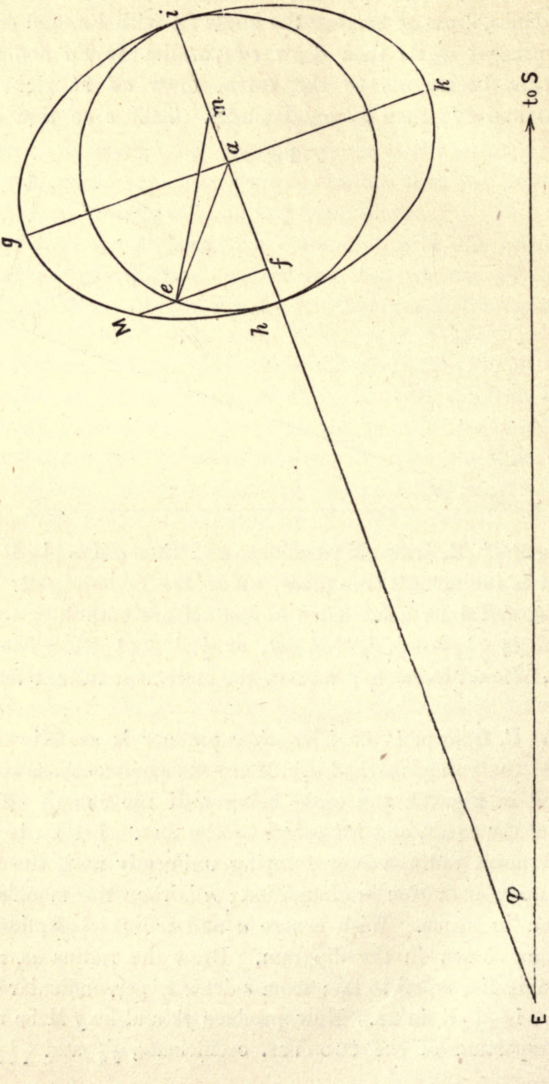
Fig. 30.



represent $\frac{1}{136}$R, draw eM parallel to ad; then $d\text{M} = \frac{1}{136}\text{R}\cos 2\phi$, and M is the moon's true place. Now $ec = (\frac{1}{96} - \frac{1}{136})\text{R}$. Bisect it in b, and draw bM. Then be and bM are both $\frac{1}{2}(\frac{1}{96} - \frac{1}{136})\text{R}$, and ab is $\frac{1}{136}\text{R} + \frac{1}{2}(\frac{1}{96} - \frac{1}{136})\text{R}$, or $\frac{1}{2}(\frac{1}{96} + \frac{1}{136})\text{R}$. The angle $\text{M}be = 2dca = 2\text{E}ac = 4\phi$; whence the statement in text follows.

NOTE C, from p. 109.—The other manner is as follows. (In Fig. 31 the points marked a, e, M are the same as those similarly marked in Fig. 30, the scale being still the same.) E is the place of the earth, and ES points to the sun. Let Ea be R, the moon's mean radius-vector rotating uniformly with the moon's mean angular motion in elongation; a is then the moon's mean position in space. With centre a and radius $\frac{1}{136}$R, describe a circle, as shown in the diagram. Draw the radius ae, making the angle Eae equal to 2ϕ; from e draw ef perpendicular to Ea; then fe is $\frac{1}{136}\text{R}\sin 2\phi$. Now produce fe and let fM be to fe in the proportion of the two Var. coefficients, $\frac{1}{96}$ and $\frac{1}{136}$ (very

Fig. 31.



nearly as 7 to 5); then M is the position of the moon, for her assumed mean elongation ϕ; and af is $\frac{1}{136}$R $\cos 2\phi$, or (since Ef differs only insensibly from EM) the change in length of the moon's radius-vector, for assumed ϕ. The point M describes round a, as centre, an ellipse which is as though it were rigidly attached to the line Ea, and therefore rotates about its centre once in a month progressively, relatively to ES regarded as fixed, or in the same direction as that of the moon's revolution round the earth; its semi-axes major and minor ag and ah being $\frac{1}{96}$R and $\frac{1}{136}$R, respectively, and to each other in the proportion of the two Var. coefficients. As ae rotates round a, relatively to ah, with twice the moon's mean angular velocity in elongation, M describes the whole ellipse in half a synodical month; its motion therein being retrograde, or in the direction contrary to that of the moon's revolution round the earth. At the times of both syzygies the moon is at h in the ellipse, and nearest the earth; and at the times of both quadratures she is at i, and farthest from the earth; and when in octants she is at g, or at k, and at her mean distance. This is very approximately so; but only because the semi-axis-major of the ellipse is so small relatively to the moon's mean distance from the earth. The ellipse has necessarily been drawn in the diagram vastly too large in proportion to Ea, the moon's mean distance.

It will be observed that the components of M's motion parallel to hi, and to gk, are simple harmonic motions, and that the moon describes the ellipse, relatively to its (rotating) principal axes, as she would a stationary ellipse under the action of a central force varying directly as the distance; she therefore describes the rotating ellipse with a constant areal velocity.

The very approximate correctness of this is due to the fact that the dimensions of the ellipse are so small relatively to the moon's mean radius-vector Ea.

To obtain a graphical representation of the solar disturbing forces, let us return to the expression for the radial force, viz., $\frac{3SR^3}{2ED^3}(\cos 2\epsilon + \frac{1}{3})$, and to that for the tangential force,

viz., $\frac{3SR^3}{2ED^3}\sin 2\epsilon$; the earth's attraction on the moon being unity. Let us substitute in these ϕ for ϵ, which will involve an exceedingly small inaccuracy. Then, to use Fig. 31 for a different purpose, if we take the radius of the circle therein to represent the common coefficient in these expressions, and if *am* be one third of said radius, *fm* will represent, on the same scale, the radial disturbing force for assumed ϕ, and *ef* the tangential disturbing force, and *em* will represent, *quam prox.*, both in magnitude, direction, and sense (but of course not in position), the whole disturbing force acting on M. Its magnitude is said radius of the circle $\times \frac{1}{3}\sqrt{10+6\cos 2\phi}$; and its inclination to E*c* is $\tan^{-1}\frac{\sin 2\phi}{\cos 2\phi+\frac{1}{3}}$.

Note D, from p. 111.—This can be seen as follows from equation (5). R and C being as in text, let ρ be the radius of curvature at the points in question, and e an indefinitely small elongation of the moon, for which equations (5) and (3) coincide. Now $\rho = \text{arc}^2/2$ (fall from tangent). But, for syzygies,

$$\text{arc}^2 = R^2(1-C)^2\sin^2 e,$$

and

$$2\text{ fall} = 2\left\{\frac{R(1-C)}{\cos e} - R(1-C\cos 2e)\right\}$$

$$= \frac{2R}{\cos e}\left\{1-C-\{1-C(2\cos^2 e-1)\}\cos e\right\}.$$

By the addition and subtraction of $C\cos e$ within the large parentheses this becomes

$$\frac{2R}{\cos e}\left\{(1-C)(1-\cos e)-2C(1-\cos^2 e)\cos e\right\}.$$

$$\therefore \rho = \frac{R\cos e}{2}\left\{\frac{(1-C)^2(1-\cos^2 e)}{(1-C)(1-\cos e)-2C(1-\cos^2 e)\cos e}\right\}.$$

Dividing above and below by $1-\cos e$, and then making $e=0$, we obtain the result in text.

In an ellipse whose semi-axes major and minor are a and b, respectively, the radius of curvature at the apses is $\frac{b^2}{a}$, and that at the ends of the axis-minor $\frac{a^2}{b}$. Therefore if the Var. orbit were an ellipse with the same principal axes, ρ would be at syzygies $R\frac{(1+C)^2}{1-C}$, and at quadratures $R\frac{(1-C)^2}{1+C}$. Taking R as unity, and C as 0·00738, we find the following values for ρ :—

	At syz.	At quadr.
In Var. orbit	1·0230,	0·9787.
In ellipse	1·0224,	0·9781.

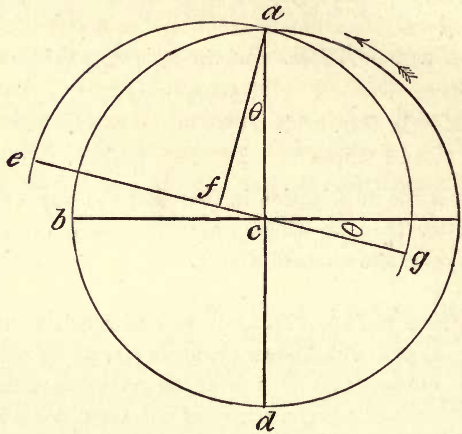
which verifies the anticipation in text that the Var. orbit is very slightly flatter than an ellipse, with the same principal axes, both at syzygies and at quadratures.

NOTE E, from p. 115.—This will be sufficiently seen from the following. It is a well-known principle (confining our attention now to the ellipse) that if a body be projected from a given point in presence of a given centre of attractive force having the law of gravitation, with a given velocity not too great for the description of an ellipse about that centre, the ellipse described by the body will have the same axis-major, whatever be the direction of discharge.

Now let the body be at first describing a circular orbit *abd*, Fig. 32, with radius r about the centre of force c, in the direction of the arrow. At the point a the direction of the body's motion is changed outwards, say by the angle θ; and it proceeds to describe an ellipse *gae*, of which a focus is at c, and whose semi-axis-major is equal to r, the radius of the circle. Since ca, drawn from the focus, is equal to the semi-axis-major of the ellipse, a is at the end of the axis-minor thereof. Consequently a line drawn through c parallel to the new direction of motion at a gives the direction of the axis-major containing the apogee of which we are in quest. The geocentric angular distance of the apogee from a is $90°-\theta$; and f being the centre

of the ellipse the height of the apogee above the circle is equal to cf, or $r \sin \theta$. The same Fig. can be used (by supposing the body to be describing the circle in the opposite direction) to show that if the change of the direction of the body's motion at a had been

Fig. 32.



downwards, and of magnitude θ, the resulting perigee at g would be $90° + \theta$ from a, and fall below the circle at that point, $r \sin \theta$.

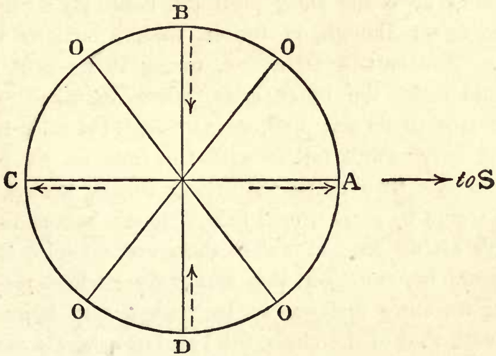
Therefore, whether the deflection at a be upwards or downwards, the new orbit will be an ellipse whose axis-major is $2r$, and axis-minor $2r \cos \theta$; and if θ be very small, the apses are distant from a by $90°$ very nearly.

We need not pursue this any further, because the actual conditions are slightly different from what we have just considered. The moon is not simply deflected outwards without change of velocity by the radial forces near syzygies; though the condition nearly approaches this, as the radial disturbing forces are so very small. But the difference of conditions is evidently in favour of an apogee both higher and nearer to quadrature than what we have been contemplating. The deflection does

not indeed occur at a single point; but it may be regarded as the result of a large number of exceedingly small, outwardly directed, radial impulses, whose magnitudes are very nearly equal at equal distances on each side of syzygy. The whole effect is, therefore, different from that of a single impulse at syzygy equal to the sum of the others; but, as regards our present purpose, the difference is quite unimportant.

NOTE F, from p. 116.—We have seen, p. 106, that the radial disturbing force vanishes at the four points of the moon's orbit distant 54° 44′ from syzygies, marked OOOO in Fig. 33. The arc BO is slightly less than two thirds of AO; and, the changes of the moon's velocity being small, her times of describing OB

Fig. 33.



and AO are still more nearly in the same ratio. Again, we have seen above that the inward radial force at B is half the outward radial force at A; and therefore, as it is easy to see, the average inward force on each side of quadrature is somewhat less than half that on each side of syzygy.

In consequence of the outwardly exceeding so much the inwardly directed radial impulses, the earthward pull on the moon is, on the whole, diminished; and therefore the mean distance of the moon from the earth is by them increased.

NOTE G, from p. 118.—It will be found from simple considerations similar to the above, *mutatis mutandis*, that if the gravitation attraction varied, not according to its actual law, but according to that other law of force so frequent in nature, viz., directly as the distance, the solar disturbing forces, if turned on to act on an originally circular lunar orbit, would make that orbit into what might be called an "ellipse," with the earth at its centre, whose axis-major, in the line of the second and fourth octants, would be continually increasing, whilst its axis-minor would be continually decreasing (more rapidly), until the moon came into collision with the earth. The instantaneous ellipse would be always writhing; especially towards the conclusion of its history. In this case there would be no baffling action between the solar and the terrestrial forces. The solar tangential forces would have the same positions, relatively to the sun, as they have now; though, of course, their directions would be reversed. The earth's attraction, owing to its now supposed law, would make the lunar orbit, when disturbed, a central ellipse, if free to do so; and, as is evident, the solar tangential disturbing forces would fall in with this and go on increasing the ellipticity. The radial disturbing forces, always directed inwards, would be proportional to the moon's radius-vector, like the earth's attraction, and would therefore conspire therewith. There would be, moreover, this seemingly curious result, that, supposing the sun's distance to be always very large in comparison with that of the moon, the Var. forces would not sensibly alter with the sun's distance; instead of being, as they actually are, inversely proportional to the cube of that distance.

NOTE H, from p. 119.—This will be seen thus. In Fig. 34, E is the earth's place, r the moon's radius-vector for the point a, in question, in the Var. orbit, of which the curve ac represents a portion, and $d\epsilon$ an indefinitely small alteration of ϵ, the moon's elongation. Let θ be the angle between the radius-vector and the curve at a, or the tangent thereto. Then, R, the moon's

mean radius-vector, being taken as unity, we have

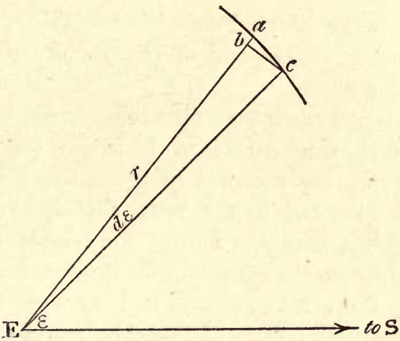
$$r = 1 - C\cos 2\epsilon \text{ (p. 110) and}$$
$$dr = +2C\sin 2\epsilon d\epsilon.$$

The earth's attraction at the point in question is $\frac{1}{r^2}$; the mean attraction being unity. But

$$\frac{1}{r^2} = 1 + 2C\cos 2\epsilon, \textit{ quam prox.}$$

This multiplied by $\cos\theta$ is the terrestrial tangential force.

Fig. 34.



Now $\cos\theta = \cot\theta$, *q. pr.*; as θ differs so very slightly from a right angle. Draw cb perpendicular to Ea. Then $\cot\theta = \frac{ab}{bc} = \frac{dr}{rd\epsilon}$
$= \frac{2C\sin 2\epsilon d\epsilon}{(1 - C\cos 2\epsilon)d\epsilon} = 2C\sin 2\epsilon(1 + C\cos 2\epsilon)$, *q. pr.*

Therefore the terrestrial tangential force is

$$(1 + 2C\cos 2\epsilon)2C\sin 2\epsilon(1 + C\cos 2\epsilon)$$
$$= 2C\sin 2\epsilon(1 + 3C\cos 2\epsilon), \textit{ q. pr.},$$
$$= \tfrac{1}{68}\sin 2\epsilon(1 + \tfrac{1}{45}\cos 2\epsilon). \quad \text{Q. E. D.}$$

This is never less than $\frac{1}{68}\sin 2\epsilon$; while the solar tangential force is $\frac{1}{120}\sin 2\epsilon$ (p. 105). Therefore the proportion of the terrestrial to the solar tangential force, at any point in the lunar Variation orbit, is always at least as high as 120 to 68, or as 7 to 4, very nearly.

CHAPTER VIII.

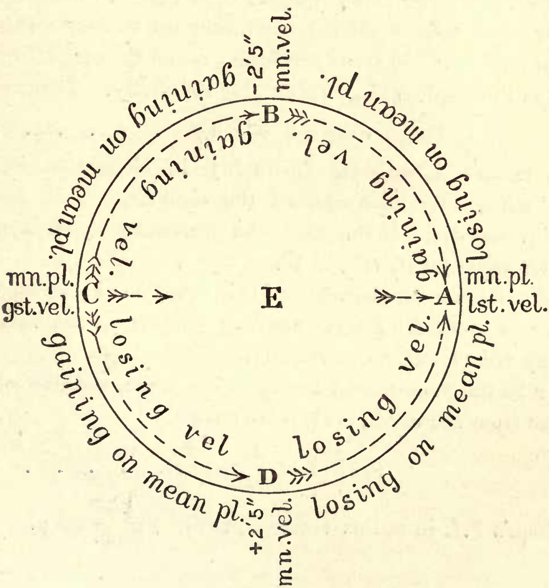
THE MOON'S PARALLACTIC INEQUALITY.

We now turn to the moon's Parallactic Inequality, whose scheme of solar disturbing forces and of changes of velocity, &c., are indicated in Fig 35.

In the Variation scheme the disturbing forces, both tangential and radial, on the sunward side of the moon's orbit and those on the opposite side are regarded as equal, which, however, they evidently are not; the former being slightly greater, and the latter slightly less, than the mean. To remedy this we must now add to those on the sunward side the necessary differential forces having the same direction; and we must subtract from the Var. forces, both radial and tangential, on the off side of the orbit, the same differential forces; or, in other words, join with them the said differential forces having the contrary direction. These constitute the P.I. forces, with which we now have to do. The inwardly directed radial disturbing forces *at* B and D, in the Variation orbit, Fig. 26, are not affected by the difference between the sunward and the other side of the Var. orbit, and we have put no arrows at those places in Fig. 35. The sun being over A, the disturbing forces, with which we now have to do, are represented by the arrows drawn with broken lines. The terrestrial tangential forces, to be mentioned presently, are omitted to avoid confusion. They are directed oppositely to the solar ones; they are, however, at their maximum at both quadratures, while the solar ones vanish at those points. All vanish at syzygies.

The P.I. forces, being second differences, or differences between what were themselves only differential forces, are exceedingly small. It is easy to find that, taking the earth's mean attraction on the moon as unity, the solar P.I. tangential force $= 6\frac{SR^4}{ED^4} \times (\sin \epsilon - \sin^3 \epsilon)$, and that the radial force $= 6\frac{SR^4}{ED^4}(\cos^3 \epsilon - \frac{1}{2} \cos \epsilon)$; all the letters here having the same meaning as they have in the expressions for the Variation forces in p. 105. (See NOTE A.) The

Fig. 35.



greatest P.I. forces are the radial ones at syzygies; and these are only 1/23,300th of the earth's mean attraction on the moon. They are about 1/259th of the Var. radial forces at the same points; these being also at *their* maximum at those points.

The P.I. perturbations, like the Var. ones, can be calculated and considered by themselves. Since these two sets of disturbances are both very small, they can be combined like two sets

of "small oscillations" by simple superposition. We shall, therefore, as we did with the Var., neglect now all other deformations of the moon's orbit and inequalities in her motion, and suppose the moon's undisturbed orbit and the sun's relative annual orbit round the earth to be both circular, and the angular velocities therein uniform, and the P.I. forces to be the only disturbing forces acting on the moon. The lunar orbit round the earth resulting from this we shall call the P.I. orbit.

Let ϵ be the moon's actual elongation, as above, in the pure P.I. orbit, and ϕ her mean elongation, or that in the undisturbed circular orbit, both reckoned eastwards up to 360°; the sun's angular motion of apparent revolution round the earth being supposed, as in Chapt. VII., constant, for simplicity. Then we have

$$\epsilon=\phi-2'\,5''\sin\phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The moon's Parallactic Inequality in elongation is, then, $-2'\,5''\sin\phi$. We have adopted the coefficient $2'\,5''$ from the latest investigations of the American astronomers; Hansen gives a smaller value for it, viz., $2'\,1''$.

Observations might be made on this equation (1) corresponding to those in NOTE A of the preceding chapter on the Variation; but they are probably unnecessary.

Let r be the moon's actual, and R her mean, radius-vector, or distance from the earth. Then we have

$$r=\mathrm{R}\left(1+\frac{1}{3520}\cos\phi\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

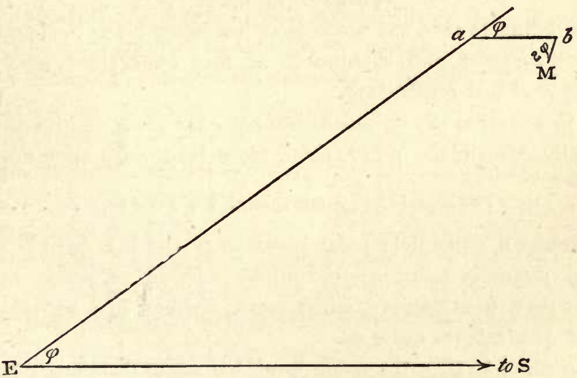
The Moon's P.I. in radius-vector is, then, $+\frac{\mathrm{R}}{3520}\cos\phi$.

From these two equations, in combination, may be derived a simple geometrical construction for obtaining the moon's position in space, for any assumed ϕ, or mean elongation. First let us note that the coefficient $2'\,5''$, in equation (1), is, in circular measure, $\frac{1}{1650}$. Whence the moon's linear departure, backwards or forwards, from the line of her mean radius-vector is

$-\frac{R}{1650}\sin\phi$, *q. pr.*; her departure from her mean distance from the earth being, as we have seen, $+\frac{R}{3520}\cos\phi$.

In Fig. 36, E is the place of the earth, and ES the direction of the sun. The construction is as follows:—Draw Ea to represent, in magnitude and position, the mean radius-vector, at some given time, whose length is 238,820 miles; SEa is ϕ, and a is the moon's mean position. Draw ab sunward, making the angle ϕ with the production of Ea (that is parallel to ES), and of magnitude to represent $\frac{1}{2}\left(\frac{1}{1650}+\frac{1}{3520}\right)$R, or 106 miles; then draw bM making the angle 2ϕ with ab (and with ES), and of length to represent $\frac{1}{2}\left(\frac{1}{1650}-\frac{1}{3520}\right)$R, or 38 miles; then M is the moon's true place for assumed ϕ. (See NOTE B.)

Fig. 36.



Thus we see that the moon's P.I. orbit, considered as described about E, and relatively to ES regarded as stationary, is a peculiar epicyclic curve; Ea is the radius of the deferent circle turning progressively with its constant angular velocity. It carries, as in Fig. 36, the line ab, which remains parallel to ES and itself, which also carries at its end the radius bM of the

epicycle, which radius rotates progressively with twice the angular velocity of E*a*.

If we take the step EF, from E sunwards, equal to *ab*, then the P.I. orbit will be, relatively to F, a simple epicyclic with the same deferent and epicycle having the same simple proportion of their angular velocities.

The movement of the moon in her P.I. orbit can be represented in another manner, which, of course, is the same at bottom, but has its own interest. It follows quite easily from equations (1) and (2). (See NOTE C.)

It is often said simply that the moon's P.I. in elongation from the sun is proportional to the sine of her elongation. The difference involved between this and equation (1) is exceedingly small and, as regards our present purpose, insensible. Let us then take leave to write equation (1) thus

$$\epsilon=\phi-2'\,5''\sin\epsilon. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

According to this, the moon is at her mean place in elongation at both syzygies, most behind it at first quadrature, and most before it at last quadrature.

If in equation (2) we substitute $\cos\epsilon$ for $\cos\phi$, the inaccuracy is again insensible. When, then, we write the equation thus—

$$r=\mathrm{R}(1+\frac{1}{3520}\cos\epsilon)=\mathrm{R}(1+c\cos\epsilon), \quad . \quad . \quad . \quad (4)$$

it becomes a convenient polar equation of the P.I. orbit referred to the (annually rotating) line of conjunction, as prime vector. This equation is always, *quam prox.*, correct, and at syzygies and at quadratures quite so.

According to this, the moon is at her mean distance from the earth at both quadratures, at her greatest distance at conjunction, and at her least at opposition. These differ from the mean by only about 67·8 miles.

The P.I. orbit, as given by equation (4), differs very little indeed from a circle which has been first shifted bodily sunwards by the distance $\frac{\mathrm{R}}{3520}$, or 67·8 miles, retaining quite unaltered

its syzygy diameter, and then drawn out at right angles to that diameter until it becomes wider by Rc^2, or only 102 feet. (See NOTE D.) The greatest width is very near, and on the sunward side of, the line of quadratures. In the drawing out, the circle becomes flattened a little at both syzygies; but *very* slightly more at opposition than at conjunction.

It is easy to obtain geometrically the radius of curvature at conjunction, viz., $R\frac{(1+c)^2}{1+2c}$, and that at opposition, viz., $R\frac{(1-c)^2}{1-2c}$; c being the coefficient $\frac{1}{3520}$. The former is less than the latter (though by only about 1·4 *inch*); but both exceed R, the mean radius-vector and radius of curvature. (See NOTE E.)

As with the Var. diagram, so now, we can fill in all the writing in the P.I. diagram, Fig. 35, p. 133, when we know simply from equation (3) that the moon is most behind her mean place at first quadrature and most before it at last quadrature; she being of course at her mean place at both syzygies. Among these conclusions let us note particularly that the moon's velocity is least at conjunction, greatest at opposition, and at its mean at both quadratures. We must return to this hereafter.

The reader will perceive better the differences between the P.I. and the Var. diagrams by comparing them for himself, than by reading our description of them. We may, however, draw his attention to the following point. If we start from C in both diagrams, we shall find that, as regards the writing only, the four reaches, or divisions (constituting one half) of the Var. orbit from C to A, correspond, respectively, to the four reaches (constituting the whole) of the P.I. orbit.

An elegant explanation of the production of the Parallactic Inequality by the disturbing forces now in question will be found in Airy's *Gravitation*, p. 68, which we shall not reproduce here. (See NOTE F.)

The existence of this lunar inequality was pointed out by Newton. It is very interesting to find that he had determined

dynamically its amount with a wonderful closeness of approximation; though it had not been, in his time, detected by observation. The value that he gave to the coefficient was $2' 20''$; this is too large; the reason being that he went on the supposition that the sun's parallax was $10''$, which we now know to be greater than the true magnitude.

This brings us to the connection between the sun's parallax and this lunar inequality, which was named by Newton from its dependence on the ratio between the sun's and the moon's parallax. Newton could only derive the magnitude of the P.I. longitude coefficient from the then supposed magnitude of the sun's parallax. But now that the said coefficient is obtainable by observation, it can be used for solving the inverse problem, viz., obtaining the parallax of the sun. Different formulæ have been given for the connection of the two quantities, which formulæ are, of course, very approximately the same at bottom. They come to this, that under the actual conditions of magnitude of the quantities concerned the sun's parallax is almost exactly one fourteenth of the P.I. longitude coefficient.

But besides this, the interest of this lunar inequality is greatly increased by its having several apparent paradoxes connected with it. This circumstance has not attracted the attention it deserves; and the neglect of it has given rise to certain erroneous statements. Of the seeming paradoxes we shall mention five, to be dealt with by eqns. (3) and (4) and Note E.

1. Since the Var. forces produce the inequalities indicated in Fig. 26, p. 105, the reader might naturally expect that the increase of those forces in the sunward half of the moon's orbit, by the addition of the similarly directed P.I. forces, should increase the inequalities in the moon's motion there; and, correspondingly, that the diminution of those forces on the off side of the orbit, by applying to them the oppositely directed P.I. forces, should diminish the inequalities *there*. But these are both the reverse of the truth.

2. Since the Var. orbit is compressed at A and C by the influence of the Var. forces, the reader would naturally expect that the just mentioned increase of the forces on the sunward side of the orbit, by the addition of the P.I. forces, should increase the compression there; and, correspondingly, that the diminution of the forces on the off side of the orbit, by the subtraction of the P.I. forces, should diminish the compression *there*. But these are both the reverse of the truth.

3. When the reader has teachably accepted, from equation (4), the position that the effect of the P.I. forces is to elongate the originally undisturbed orbit towards the sun and to compress it on the opposite side, he will loyally endeavour to carry out his newly acquired knowledge, and will conclude that the orbit is more flattened on the off side from the sun, and less flattened on the side next the sun, than elsewhere. But as respects the sunward side this is the reverse of the truth. (See again NOTE E.)

4. The reader will most naturally, and even commendably, think that the moon would be gaining, or losing, velocity, in the P.I. orbit, according as the P.I. tangential disturbing forces are directed with, or against, her motion, respectively; and therefore that her velocity is greatest at conjunction. He will be confirmed in this expectation by seeing that such happens to be the case in the Var. orbit; see Fig. 26. He will think also that the moon's velocity must be least at opposition, since she has been opposed by the solar tangential force all the time of her passing from conjunction to opposition. But all this is the reverse of the truth. The moon always quickens or slackens her pace in apparent defiance of the solar P.I. tangential forces. (See NOTE G.)

5. It would be reasonable enough to expect that since the P.I. forces are proportional to the inverse fourth power of the sun's distance, the P.I. in longitude should also be proportional to the same, or at least pretty nearly so. Such, however, is by no means the case. The P.I. in longitude is inversely propor-

tional to a complicated function of the sun's distance; which function is much nearer to the cube than to the fourth power thereof. So that if there were an alteration of the sun's mean distance, the P.I. and the Var. in longitude would change at not very different rates.

We shall mention further on what some might regard, at first sight, as another apparent paradox; but it is only kinematical in character.

The reader, if he accepts our statements, will probably begin in despair to imagine that the name which has been given to this scheme of lunar inequalities is a mistake for "Paradoxical Inequality."

The general explanation of the above apparent paradoxes is two-fold. In the first place, as we have noted already, the solar disturbing forces, whether of the Var. or the P.I., produce their respective inequalities of the moon's motion in elongation in two quite different ways, viz., by their direct local influence on the moon's velocity in the various parts of her orbit, and also by what we may call their indirect general influence in deforming the orbit, and thus creating tangential components of the earth's attraction on the moon, which are actually greater than the solar tangential forces. In the second place, unlike the case of the Variation (see p. 118, above), the P.I. system of solar disturbing forces has but one axis of symmetry passing through the earth, that of the line of syzygies. This involves a most important difference as to the dynamics of these two schemes of lunar inequalities, as considered in NOTE F.

It so happens, as we have seen, that, in the case of the Var. orbit, the created terrestrial tangential forces always act along with the solar tangential forces; and thus, in the usual elementary treatment of the Variation, they are not prominently noticed, or are even disregarded altogether; although they are, even in that orbit, more important than are the solar ones themselves, as to their direct local action.

But in the case of the P.I. orbit, the relative importance of the terrestrial tangential forces is much more striking, for two reasons. The small solar P.I. forces have, by accumulation of effects, deformed the orbit to such an extent (very small, however, absolutely) that the terrestrial tangential forces created thereby are much greater, proportionally, than the solar tangential forces. The former are always equal to the latter multiplied by $3{\cdot}31 \sec^2\epsilon$. They are, therefore, never less than 3·31 times as great as the latter; and when the moon is not far from quadratures, very much more, proportionally, than this. (See Note H.) And as the greater terrestrial, act always against the smaller solar, ones, the singular result follows that the inequalities in the moon's velocity and in her elongation, now under consideration, are the opposite of what the solar tangential forces, with which we are now engaged, are endeavouring to effect by their direct local action. So that, paradoxical as it sounds, it is strictly true that the terrestrial tangential forces are the *immediate* cause of the moon's P.I. in elongation, and that this lunar perturbation would be greater, but for the hindrance of the direct local action of the solar tangential forces. Thus the moon's Parallactic Inequality presents a peculiarly interesting dynamical problem.

Note A, from p. 133.—The verification of these expressions for the disturbing forces, though of a simple character, is a little troublesome. If the reader should undertake it, let him beware not to stop at the first approximation, which would give the numerical factor 7 instead of 6; which latter is sensibly accurate. The value of the coefficient $6\frac{SR^4}{ED^4}$ is 0·0000858.

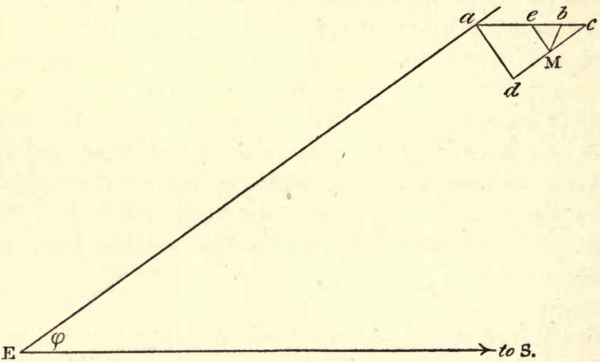
These expressions show that the P.I. forces are, *quam prox.*, inversely proportional to the fourth power of the sun's distance from the earth. It might seem just at first sight that they are also directly proportional to the fourth power of the moon's distance from the earth. But they are proportional only to the

second power thereof. The R^4 comes in on account of the earth's mean attraction on the moon being here taken as unity.

The trigonometrical factor in the expression for the tangential force can be written $\frac{1}{2}\sin 2\epsilon \cos \epsilon$; that for the radial force can be written $\frac{1}{2}\cos 2\epsilon \cos \epsilon$. This gives the interesting result that, at the elongation ϵ, the tangential force divided by the radial force $= \tan 2\epsilon$.

NOTE B, from p. 135.—In Fig. 37 the points marked a, b, M, are the same as those similarly marked in Fig. 36. Ea being the moon's mean radius-vector, necessarily drawn vastly too

Fig. 37.

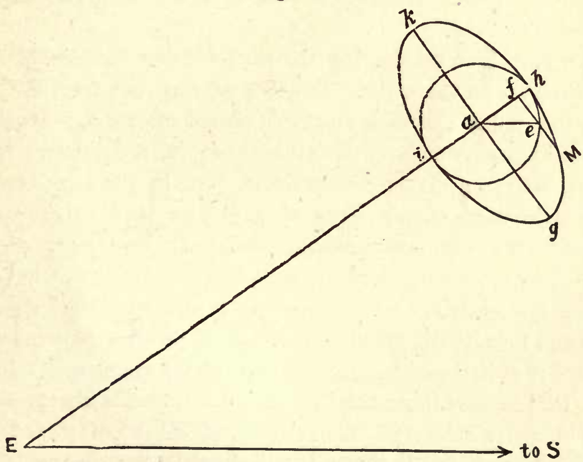


short relatively to the other lines, and a the moon's mean place, draw ac making the angle ϕ with the production of Ea (and parallel with ES) to represent $\frac{1}{1650}$R; then draw cd parallel to Ea, and sensibly pointing backwards to the earth; draw ad at right angles to Ea and cd; then ad is $\frac{1}{1650}\mathrm{R}\sin\phi$. Take e so that ae may represent $\frac{1}{3520}$R; draw eM parallel to ad; then dM is $\frac{1}{3520}\mathrm{R}\cos\phi$, and M is the moon's true place. Now ec is $\mathrm{R}\left(\frac{1}{1650}-\frac{1}{3520}\right)$. Bisect it in b, and draw bM. Then be and bM are both $\frac{1}{2}\mathrm{R}\left(\frac{1}{1650}-\frac{1}{3520}\right)$, and ab is $\frac{1}{3520}\mathrm{R}+\frac{1}{2}\mathrm{R}\left(\frac{1}{1650}-\frac{1}{3520}\right)$, or

$\frac{1}{2}R\left(\frac{1}{1650}+\frac{1}{3520}\right)$. The angle $Mbe=2Mcb$, or 2ϕ; whence the statement in text follows.

NOTE C, from p. 136.—(See Fig. 38.) As before, E is the place of the earth, and ES the direction of the sun, and the points marked a, e, M are the same as those similarly marked in Fig. 37. Let Ea be the moon's mean (both as to position and magnitude) radius-vector; so that a is the moon's mean position. With centre a and radius $\frac{1}{3520}$R, describe the circle

Fig. 38.



shown in the Fig. Draw the radius ae parallel to ES, making the angle hae equal to aES, or ϕ. Through e draw fM perpendicular to Ea; then af is $\frac{1}{3520}$R cos ϕ, or (since Ef is not sensibly different from Ee) the change in the length of the moon's radius-vector, for assumed ϕ; and fe is $\frac{1}{3520}$R sin ϕ. Now if fM be to fe in the proportion of the two P.I. coefficients 2′ 5″, or, in circular measure, $\frac{1}{1650}$, to $\frac{1}{3520}$, which is 15 to 7, very nearly, then M is the position of the moon for her assumed mean elongation ϕ.

The point M describes, round a as centre, an ellipse which is as though it were rigidly attached to Ea, and therefore rotates about its centre once a month progressively; its semi-axes major and minor, ag and ah, being $\frac{1}{1650}$R and $\frac{1}{3520}$R, respectively; and as ae rotates, relatively to ah, with the moon's mean angular velocity in elongation (not in longitude), the ellipse is described retrogressively in a synodical month. At the time of conjunction the moon is at the point h of the ellipse, and farthest from the earth; at the time of opposition she is at i in the ellipse, and nearest to the earth; and she describes the ellipse with a simple harmonic motion relatively to each of the principal axes of the (rotating) ellipse.

We have been considering the matter from the standpoint of an observer on the earth. But it is only as seen from the earth that the moon makes a complete circuit round a. Since the above ellipse, which is described once in a synodical month retrogressively, rotates progressively once in the same time, or otherwise more simply, since ab is greater than bM, the moon never makes any circuit round a relatively to fixed space, or as viewed by a spectator looking at right angles to the plane of her orbit. She is always more or less nearly on the sunward side of a.

Here, then, is the seeming kinematical paradox, as some might regard it at first sight (only), to which we have already alluded; viz., that in describing the P.I. orbit the moon is always nearly on the same side, speaking roughly, of her mean place! The explanation of this is that a is the moon's mean place relatively only to the earth about which she is revolving.

The P.I. has, moreover, its own seeming kinematical paradox precisely similar to that of the Var. considered at p. 121.

In this we have contemplated the P.I. as existing by itself; but if we consider it as superposed on the Var., a must be regarded as the moon's position in the Var. orbit. The inaccuracy involved in doing this is quite insensible.

The P.I. disturbing forces are too complicated to be introduced with advantage into Fig. 38.

NOTE D, from p. 137.—This will appear thus:—Adopting equation (4), we have $r = R(1 + c\cos\epsilon)$; c being the coefficient $\frac{1}{3520}$. The ordinate y at any point of the lunar P.I. orbit is $r\sin\epsilon$, or $R(\sin\epsilon + c\cos\epsilon\sin\epsilon)$, which is $R(\sin\epsilon + \frac{1}{2}c\sin 2\epsilon)$. Therefore $dy = R(\cos\epsilon + c\cos 2\epsilon)d\epsilon$. For y a maximum, $\cos\epsilon = -c\cos 2\epsilon = -c(2\cos^2\epsilon - 1)$. This quadratic equation gives

$$\cos\epsilon = \pm\sqrt{\frac{1}{2} + \frac{1}{16c^2}} - \frac{1}{4c},$$

which is

$$\frac{\pm\sqrt{8c^2+1} - 1}{4c},$$

and this is c, *quam prox.*; and

$$\sin\epsilon = \sqrt{1 - c^2},$$

which is $1 - \frac{1}{2}c^2$, *q. pr.*, on account of the exceeding smallness of c. Hence y (or $r\sin\epsilon$), at its maximum, is $R(1 + c^2)(1 - \frac{1}{2}c^2)$, and this is $R(1 + \frac{1}{2}c^2)$, *q. pr.*

Therefore, taking R as 238,820 miles, that maximum diameter is longer than the syzygy diameter, or 2R, by Rc^2, or 102 feet (Q.E.D.). Said maximum diameter passes very nearly indeed through the middle point of the syzygy diameter, and consequently between the centre of the earth and the sun, and thu does not coincide with the line of quadratures.

NOTE E, from p. 137.—The radius of curvature ρ at conjunction may be obtained thus:—Let e be an indefinitely small ϵ or elongation; then we have by equation (4), for the radius-vector at conjunction, $R(1 + c)$; c being the coefficient $\frac{1}{3520}$, as in last NOTE. The radius of curvature $\rho = \text{arc}^2/2$ (fall from tangent). But

$$\text{arc}^2 = R^2(1+c)^2\sin^2 e, \text{ and}$$

$$2\text{ fall} = 2\left(\frac{R(1+c)}{\cos e} - R(1 + c\cos e)\right),$$

$$= \frac{2R}{\cos e}(1 + c - \cos e - c\cos^2 e).$$

Therefore

$$\rho = \frac{R(1+c)^2(1-\cos^2 e)\cos e}{2\{1-\cos e+c(1-\cos^2 e)\}},$$

$$= \frac{R(1+c)^2(1+\cos e)\cos e}{2\{1+c(1+\cos e)\}}.$$

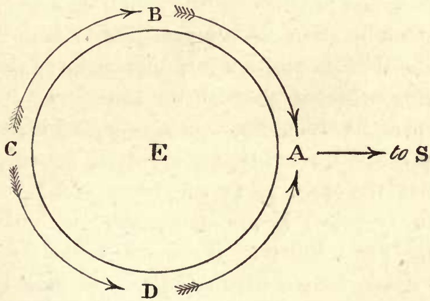
When e vanishes this becomes $R\frac{(1+c)^2}{1+2c}$. Similarly, the radius of curvature at opposition ρ' becomes $R\frac{(1-c)^2}{1-2c}$, as in text.

These evidently differ very slightly indeed from R, and from each other. Taking R as 238,820 miles, and c as 0·000284, we find $\rho'-\rho=1·39$ *inch.*

NOTE F, from p. 137.—If we gave the proof here we should have to do it *ab initio*, which would require much space. But we may make the following observations on the subject. The mode of production of the P.I. orbit is exceedingly different from that of the Var. orbit. The scheme of Var. forces is symmetrical relatively to the line of syzygies *and* to that of quadratures; consequently they go through their period of change in half a synodical lunation. But the scheme of P.I. forces is symmetrical relatively to the line of syzygies only; and consequently their period is a whole lunation. Now a focal ellipse and the scheme of changing of the gravitation forces therein are symmetrical about one axis only, viz. the apsidal, and the period of the changing forces is that of one revolution of the body about the centre of force. It is evident, therefore, that if there were nothing in the conditions of the case to prevent it, the P.I. forces, when turned on to act on an originally circular lunar orbit, would go on increasing indefinitely the deformation of the orbit, whatever the character thereof might be. A moment's consideration will show the nature of the deformation. Since the tangential forces are proportional to $\sin\epsilon - \sin^3\epsilon$, the magnitudes of those belonging to the lower two arrows in Fig. 39 vary symmetrically on each side of the quadrature D; they are equal at equal distances

on both sides of that point. Therefore, since they are so exceedingly small, their *impulses* are very nearly equivalent, for our present purpose, to a short sufficiently strong tangential impulse acting at D. Therefore, as they are acting with the moon's motion, they tend to produce an apogee at the opposite side of the orbit very near B. Similarly, as the forces belonging to the two upper arrows in the same Fig. are acting against the moon's motion, their impulses would produce a perigee very near D. The outwardly-directed radial forces on the sunward side of the orbit tend to produce an apogee near B, and the inwardly-

Fig. 39.



directed ones on the other side a perigee near D. The conditions of the focal elliptical orbit lend themselves compliantly to this; and if the disturbing forces cease to act, the deformation of the orbit would continue (with a very slight alteration). The consequence is that if the P.I. forces could continue to act, without the sun's relative revolution round the earth, the eccentricity of the orbit would go on increasing to a result which could not easily be followed out; but probably until the moon fell upon the earth; the line of apses remaining in quadratures and fixed in space. But this latter is prevented by the sun's relative annual revolution round the earth, which would diminish the eccentricity, and thereby give to the line of apses a pro-

gressive angular movement, relatively to space, which would, at first, be slower than the sun's; so that the sun would be overtaking it. By the time that the sun had overtaken it, the angular movement of the axis, which had been increasing, though all the while less than that of the sun, would be brought up to equality with that of the sun; and it would thenceforth continue pointing to the sun. Thus, owing to the baffling conditions of the sun's relative revolution round the earth, the result of the action of the disturbing forces is exceedingly different from what it would otherwise be. Nevertheless these forces are always tending to produce their own proper effect, which is to make an apogee very near B, and a perigee near D. But the very small effect that they can produce in one lunation, when compounded with that at conjunction, is only sufficient to cause the latter to be always ahead of its position in the preceding lunation, and to keep it moving progressively with the sun.

The P.I. orbit has been, for convenience, and indeed in accordance with precedent, roughly spoken of as an ellipse; it being intended that the earth is at the focus, and that the apsidal diameter (in syzygies) is the axis-major, with the apogee in conjunction. The velocity of the moon in the P.I. orbit would accord very nearly indeed with this; but the actual "ellipse" is one of a rather peculiar kind, in that its axis-major is slightly less than its width.

Note G, from p. 139.—This follows from the scheme of P.I. forces in Fig. 35, p. 133, and from what is told us by equation (3), as mentioned in p. 137, taken in connection with the principles of the apparent kinematical paradox in Chapt. VII., p. 121.

But as some persons may feel a difficulty in accepting this, it may be well to put the argument together here, though it be a little repetition. By equation (3), the moon is at her mean place at conjunction; therefore she is there moving either with greatest or least velocity—which? At the preceding quadrature D she is, by said equation, most before her mean place, and therefore

moving with mean velocity; but, since she is back at her mean place at conjunction, she has been losing velocity in the quadrant preceding conjunction. Therefore she is going with *least* velocity at conjunction. Similarly, on comparing first quadrature with opposition, we find that the moon is going with greatest velocity at opposition. Thus her velocity is at the maximum at opposition, at the mean at last quadrature, and at the minimum at conjunction; that is, she has been losing velocity all through that semi-orbit; though the solar tangential force has been all the while acting *in consequentia*, or along with her motion. Similarly she gains velocity all through the other semi-orbit; though the solar tangential force has been acting *in antecedentia*, or against her motion.

We have, so far, been content with the general law of the change of the moon's angular velocity in the P.I. orbit; but the exact law can be obtained by differentiating equation (3). The coefficient, in that equation, as expressed in circular measure, being $\frac{1}{1650}$, the actual vel. = the mean do. $\times(1-\frac{1}{1650}\cos\epsilon)$. This equation shows, at a glance, that the velocity is least at conjunction, greatest at opposition, and at its mean at quadratures; the opposite of what the solar tangential forces would cause by their immediate local action.

NOTE H, from p. 141.—In Fig. 40, E is the earth's place, the curve ac a portion of the moon's P.I. orbit, and $d\epsilon$ an indefinitely small increase of ϵ, the moon's elongation. Let θ be the angle between the moon's radius-vector and the curve at a, or the tangent thereto.

Then R, the moon's mean radius-vector, being taken as unity we have, for the actual radius-vector at a, by equation (4),

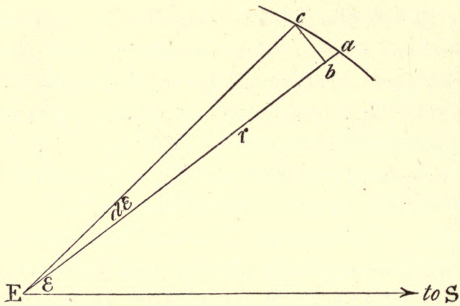
$$r=1+c\cos\epsilon;\ \text{whence}$$
$$dr=-c\sin\epsilon\, d\epsilon.$$

The earth's mean attraction on the moon being, as well as R, taken for unity, the earth's attraction at a is $\frac{1}{r^2}$, or $1-2c\cos\epsilon$,

quam prox., or sensibly 1. This multiplied by $\cos\theta$ is th earth's tangential force at a.

Now $\cos\theta = \cot\theta$, *q. pr.*, as θ differs so very slightly from a right angle. But $\cot\theta = \frac{ab}{bc} = \frac{dr}{rd\epsilon}$, and this is $-\frac{c\sin\epsilon\, d\epsilon}{(1+c\cos\epsilon)d\epsilon}$ or $-c\sin\epsilon$, *q. pr.*, or $-0{\cdot}000284\sin\epsilon$; while the sun's tan-

Fig. 40.



gential force at a is $+0{\cdot}0000858(\sin\epsilon - \sin^3\epsilon)$ (see NOTE A). Therefore the terrestrial is to the solar tangential force, at the same point with the elongation ϵ, as $3{\cdot}31\sec^2\epsilon$ to 1 (Q. E. D.). At quadratures, then, the terrestrial is infinitely greater, proportionally, than the solar tangential force; but this it can easily be, since at those points it is at its maximum, while the solar force is there zero.

Printed by TAYLOR and FRANCIS, Red Lion Court, Fleet Street.

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